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A Case of Lithotomy, with Remarks on the Effect of that Operation, and on some Cases of Fistulæ in Perinæo. By THOMAS CHEVALIER, Esq. F. L. S. Surgeon Extraordinary to the Prince Regent; and Surgeon to the Westminster General Dispensary.

[From the Medico-Chirurgical Transactions, Vol. II.]

I AM induced to lay an account of the following case before the Society, not more from the particular circumstances which the history of it is intended to record, than from the confirmation it affords of the late Dr. Austin's theory, respecting the formation and growth of stones in the urinary bladder.

Nathaniel Martin, aged fifty-nine, a stout and rather corpulent man, consulted me at the end of November, 1807, on account of extreme pain in passing his urine, with frequent provocation to void it, attended with the discharge of large quantities of mucus from the bladder. He informed me that he had several years before been attacked with dysury, and inflammation in the perinæum, which had been followed by an abscess. The abscess healed, but the symptoms of irritation in the bladder remained, and had continued to increase.

I introduced a sound, and feeling a stone very distinctly,

I did not hesitate to propose the operation, as there were no circumstances existing which appeared to me to render it unadvisable.

I performed it on the 9th of December, and extracted two stones of moderate size. He slept better that night than he had done for many months, and every thing appeared to go on favourably till the end of the second week, when I of course expected the urine would begin to pass through the urethra. The wound had constantly looked well, and was gradually closing. He was at this time attacked with a violent diarrhœa, and in a few days began to complain of pains resembling those he had experienced before the operation, and they soon became equally severe. The urine did not now continue to flow away constantly, but when a small quantity was collected in the bladder, he felt a desire to pass it, and after a severe pang, which extended along the urethra, it was discharged through the wound; and when it could be collected in a vessel, the same mucus, though much less in quantity, was found at the bottom as formerly. I had been very particular in my examination of the bladder after the operation, and had injected two large syringe-fulls of water to clear it perfectly, and therefore I was convinced no particle of stone had been left there. But as he complained of some pains in his back as well as in his bladder, I at first thought it not improbable that some small calculus had descended from one of the kidneys, and had blocked up the wound. I therefore introduced a long probe curved, through the wound, and examined it and the bladder with the utmost care. At the superior part of the incision, the probe seemed to touch particles of a gritty matter, but nothing whatever could be discovered within the bladder itself. These embarrassing circumstances were however soon explained; for in two or three days there began to be discharged through the wound, a soft kind of paste, made up of a white sabulous substance, and mucus, of which a considerable quantity was, from time to time, collected upon the dressings. He then complained of much pain in the penis, and on examining this, the urethra was found completely choked up with the same substance, but harder, of which some was taken out from time to time by a convenient instrument, as it appeared to be forced forward by every effort

to make water. At length, after several weeks spent in this manner, it ceased to be discharged, the urine then came freely through the urethra, the wound in the perinæum completely healed, all symptoms of irritation went off, and he has had no return of them to the time I am now writing this paper. (January, 1811.)

Prior to the discharge of this sandy substance, I endeavoured to relieve his pain by opiates chiefly; but when that appeared, and the diarrhœa had subsided, I ordered him the muriatic acid, with tincture of opium, first in water, and afterwards in an infusion of the uva ursi. He continued the use of the acid to the end of January. How far it might contribute to his relief I cannot say, but he thought it gave him ease, and it seemed to have a beneficial effect upon his general health.

It will be recollected to have been the opinion of Dr. Austin, deduced from a variety of experiments, that the matter composing calculi, is a deposit, furnished chiefly from the mucus secreted by the internal coat of those organs through which the urine passes; and that, therefore, this fluid contributes much less to their formation and growth in the bladder, than had been generally supposed. The quantity of mucus, which is frequently discharged with the urine of calculous patients, is, therefore, to be considered both as a cause, and an effect, of increase in the size of a stone. The stone, by its asperity, irritating the internal coat of the bladder, and exciting an excessive secretion of mucus; the mucus, on the other hand, affording an increased supply of calculous matter to crystallize upon the stone, and augment its bulk.

It is therefore probable, that a considerable quantity of calculous or sabulous matter may sometimes be supplied in a short time, when from any cause, after the extraction of a calculus, as well as under any other circumstances, the bladder is thrown into a state of irritation.

In the case I have related, this appears most evidently to have been the case. The secretion of mucus, to which the bladder had been accustomed, ceased soon after the operation; but was again renewed, in consequence of the irritation produced upon the wounded bladder by an almost incessant diarrhœa. At first it was carried off with the urine as fast as it was

secreted, and therefore no time was allowed for the sandy matter to crystallize from it; but when the wound became so nearly closed, that the bladder began to retain the urine, and of course the mucus, it was separated in considerable quantity, but was still carried off, with the mucus, in this paste-like substance, partly through the wound, and partly through the urethra. At length the secretion of this depraved mucus ceased, and all the symptoms accompanying it consequently disappeared. But, had it continued much longer, there can be little doubt that a new calculus would have formed, and a repetition of the operation would have become necessary.

It is a curious fact, which, before Dr. Austin's publication, was never explained, that a great majority of those persons who have undergone the operation of lithotomy, and have recovered, have remained free from the disease during the remainder of their lives. Some change must, therefore, have been produced in the organs secreting the fluid, whatever it be, from which the materials composing calculi are supplied. That such a change should be induced on the kidneys, seems much less probable than that it should take place in the bladder itself, on which the operation is immediately performed.

The *kidneys* continue their office after the operation, as well as before; and their experiencing little or no interruption in so doing, is considered as a favourable omen with regard to the event. It is evident, however, that the functions of the *bladder* entirely *cease* for a time; and that it enjoys, if I may so express myself, a state of absolute rest from its duties, during the whole interval which elapses between the period of the operation, and that in which the union of the wound is so far completed, as to allow the urine to accumulate in it. It then resumes its offices, both as a reservoir, and an evacuator of that fluid; but it resumes them only gradually, for the quantity it retains is at first but small, and of this a part is usually for some days discharged through the wound, and a part only through the urethra; at first with short, and afterward with longer intervals between each evacuation.

To this state of rest, and suspension of every former source of irritation, more than to any other of the causes hinted at by Dr. Austin, I am disposed to ascribe that favourable change

in the secreting surface of the internal coat of the bladder; in consequence of which, supplying afterward only healthy mucus, and this but in due proportion, the future formation of calculi, so far, at least, as it arises from superabundance, or depraved mucus, will, of course, be prevented.

That some cases should occur in which the bladder retains a considerable degree of the irritability it had acquired before the operation, so that the secretion of this mucus should continue, and calculous matter be again deposited; and that, in other cases, this irritability should be revived by the descent of new stony concretions from the kidney, which speedily become too large to be transmitted along the urethra, and thus reproduce the disease, cannot appear wonderful. It is rather surprising, that the proportion of such cases is not much greater than we really find it. It may also appear justly surprising, that the irritability of the bladder should not, in some cases, be *augmented* by the wound it receives in the operation. This is extremely rare in those cases where the patient recovers: when, as is sometimes unfortunately the case, the bladder, instead of feeling more relieved by the removal of the stone, than injured by the wound made in the operation, takes on a high degree of increased irritation: this usually commences almost from the moment of the operation, and the case is likely to terminate fatally. But if the alarming symptoms subside, and the patient afterward recovers, the bladder returns to as tranquil a state as if they had not occurred; which they are most apt to do when the patient, at the time of the operation, is in other respects in a state of high health and vigour, and the constitution has been but little affected by the disease.

The state of the bladder, however, which these symptoms denote, is not a state of mere inflammation of that organ, and is also to be distinguished from an attack of peritonitis. It appears much to depend on the shock given to the nervous system in the operation, and communicated to every part of the body; but of which the bladder, from its wounded, and previously morbid state, partakes more than any other. On the dissection of such cases, the local inflammation has sometimes been found to be slight; but it is well known, that the influence of any important, and, sometimes, even of a comparatively un-

important accident, or operation, is such, as frequently to derange the whole nervous system in a similar manner, and, for a time, to disturb every function, and deprave every secretion, in the animal economy.

That, however, an attack of common inflammation of the bladder may sometimes prove ultimately beneficial, by destroying a state of previous morbid action, the following case renders highly probable.

George Hunter, a boy about ten years of age, had frequent obstructions to the passage of his urine, by small cysts, or rather concave films, of an opake membranous appearance, which were afterwards voided. As his fits of pain were sometimes severe, it was thought highly probable there might be a stone in the bladder; and, under this impression, he had been repeatedly sounded by my judicious friend, the late Mr. Ford, but without any calculus having been discovered. When I succeeded that gentleman, as Surgeon to the Westminster General Dispensary, where the boy had been received as a patient, he came of course under my care. Shortly afterwards he was attacked with a severe fit of dysury, which was followed by violent inflammation of the bladder. It was necessary to draw off his urine from time to time, which contained a considerable quantity of purulent matter. On one occasion, when I had introduced an elastic gum catheter, I felt distinctly what appeared to me to be a stone: it was also felt by one of my pupils, who was with me, and also by the boy's father, who, on hearing me say I had found a stone, wished to satisfy himself of the fact, and was much pleased that the cause of his child's sufferings appeared at last to be detected, and a prospect afforded of their being terminated at a fit time by an operation. I was never after, however, able to distinguish it again, either with the flexible catheter, or a metallic instrument. But, when he was recovering from the attack above mentioned, he one day voided a piece of coagulable lymph, of the same form as the films which had used to come away, but of much more solidity, and containing calculous matter on both its surfaces, which was easily rubbed off. After this he felt much relieved. He once or twice brought away some small films resembling the former, but he had no return of the symptoms which before

distressed him. After some time he went to Antigua, where he now is. I have heard of him twice since his residence there; and, in his letters, he has stated himself to remain perfectly free from his complaint.

In this instance it appears most probable, that the little cysts or films, which he had been used to void, were portions of coagulable lymph, thrown out and detached, from time to time, from some small part of the bladder, or of the pelvis of the kidney, in contact with which some calculous matter had been deposited. When violent increase of action, by inflammation, took place, the calculous matter was dislodged, and the cause of the disease removed. However, as it did not return, a salutary change must also have been produced on the part primarily affected.

But in the most favourable cases of lithotomy, no particular inflammation of the internal coat of the bladder appears to follow the operation. The change which takes place in it must, therefore, depend on some other cause; and seems chiefly owing to the quiescent state in which the bladder remains for many days after the operation, and by which it is enabled to recover an healthy condition. In this it will probably continue, if no fresh cause of irritation be applied to it. I am the more disposed to believe I have not ascribed too much to this circumstance, from the benefits so often derived in other parts, which have been long in a state of morbid action, when their functions can be a while suspended, and from the facility that suspension affords to the successful prosecution of the treatment of their diseases.

In many diseases of the extremities, the necessity of this repose is obvious, and, fortunately, so is often the possibility of attaining it. But what shall we say to diseases of the stomach, of the urethra, and of other parts, to the functions of which it does not at first seem probable that any material suspension can be admitted.

Yet in several very obstinate cases of fistulæ in perinæo, particularly in one which had arisen from a violent contusion, and was accompanied with great induration in the perinæum and scrotum, from repeated inflammations and abscesses which had formed, and where, from the consequent obliquity and irregu-

larity of the canal of the urethra, added to its extreme irritability, little or no progress was made by the bougie, I was led to recur to the principle I have mentioned, and to consider whether some advantage might not be gained, if the treatment were *commenced*, by making such an opening into the urethra, behind the seat of the obstruction, as would allow the whole of the urine to pass through, without getting to the diseased portion of the canal, and insinuating itself into the sinuses that communicated with it. By thus keeping the parts without their accustomed stimulus, and allowing them to remain in a state of comparative rest for some days, I hoped so to diminish their irritability, swelling, and hardness, as to attain greater facility in carrying on the requisite treatment by the bougie and catheter. I was the more strongly inclined to expect success from this measure, from having observed the beneficial change which had almost immediately taken place in the state of the urethra, in two cases where I had found it necessary to puncture the bladder through the rectum. The result of such an opening into the urethra, as a preliminary step in the treatment, fully equalled my expectations. The parts having their chief source of irritation for a time withdrawn, in a few days became more yielding; the sinuses which were produced were consequently more easily traced through all their irregularities; way was more readily made for the passage of an instrument; and sufficient access to the bladder was soon obtained for the passage of a full sized catheter. The old sinuses were then readily traced and laid open, so as to make them granulate from the bottom and heal.

It has not been unusual to make such an opening into the urethra, when, in consequence of its being torn, or ulcerated, either from external violence, or in cases of stricture, the urine has escaped into the cellular membrane; in which instance it becomes immediately necessary to prevent, or stop, the mortification, which arises from the diffusion of this pernicious fluid. But the propriety of it, in those cases to which I have before alluded, has not been, I think, sufficiently considered; at least, I do not know that it has been proposed by any writer on these diseases. But the arguments in its favour, and the success I have experienced from it, make it appear to me deserving the attention of surgeons.

It must, however, be observed, that the indurated and thickened state of the perinæum, and the contraction of the urethra, render it often rather a difficult matter to find the canal; and therefore a free division of the integuments, in the first instance, will be prudent. And, indeed, by a free incision through the hardened integuments, considerable advantage may be gained. For, in some cases, the urethra itself is not primarily diseased to any great extent, but its canal is made irregular, and its internal surface crumpled, as it were, in consequence of the mischief around it, so that, in several instances, where I have removed the penis and bladder after death, although the smallest instrument could not be passed without great difficulty before, yet, when they were detached, a large catheter has readily gone on into the bladder.

As a remarkable example of this fact, I may mention a servant of the late Mr. Watken, who had laboured under fistulæ in perinæo for twenty years, during which time, he informed me, a bougie had been once only passed into the bladder, and that was by the late Mr. Hunter. On the last occasion in which I was sent for to visit him, he not only suffered from disease of the perinæum and bladder, but was evidently consumptive, and fast approaching to dissolution. I made various attempts to introduce an instrument into the bladder, and succeeded once in passing a very small bougie. No particular relief followed: indeed, the parts seemed rather irritated than benefited by it. On his death, I fully expected to find the urethra almost closed up for some considerable extent; but when I had taken away the penis and bladder, and the urethra was thus disengaged from the hardened cellular membrane and skin, a sound readily passed on to the bladder; and, on laying open the canal, I found no regular stricture had been formed, but that it was a little bent aside at that part; and for near two inches, from the commencement of the bulb to the prostate gland, its surface had the appearance of being crumpled or puckered into folds; which, with the indurated state of all the surrounding parts, accounted both for the difficulty of passing a bougie, and also for his being able to get the urine away with more ease than could have been expected, under so much apparent obstruction.

I have, therefore, no doubt that such a state of the urethra,

accompanying fistulæ in perinæo, is more commont han is generally supposed, especially where the disease has originated from external violence, and that it is often confounded with common Stricture. In such cases a free division of the diseased integuments down to the urethra, in performing the operation I have recommended, must tend materially to relieve that portion of it, by setting it in some degree at liberty; and by this, connected with the opening into the canal behind the diseased part, which will prevent the urine from forcing itself into the sinuses, a material advantage will be early gained, and a great hindrance to the treatment removed. The constitution, also, will sooner feel relief, in consequence of that which is afforded to the diseased parts; those medicines, which may be required to restore the general health of the patient, may therefore be exhibited with less interruption; and the cure will thus be more speedy, and probably more complete.

A Memoir upon the Organs of Absorption in Mammiferous Animals; read at the Institute, on the 7th of August, 1809, by M. MAGENDIE, Doctor of Medicine of the Faculty of Paris, Professor of Physiology, &c. *The Experiments conducted by* Doctors MAGENDIE and DELILE.

[From the New London Medical and Physical Journal, for August 1812.]

AMONG the facts which I had the honour to report to the Class, in a memoir upon the upas tieuté, the nux vomica, and the bean of St. Ignatius, there is one which appeared to me worthy of more particular attention; I mean the readiness with which those poisonous matters are absorbed and introduced into the sanguiferous system. It must be recollected, that it scarcely requires twenty seconds to convey these poisons from the peritoneal cavity to the spinal marrow.

The generally received ideas relative to the organs of absorption, do not admit of a doubt that the lymphatic vessels are the agents for conveying these poisons into the sanguiferous system. Thus, in the experiment where the poison was introduced into the middle of the thigh of an animal, there was but one way of

explaining its absorption; it must necessarily be admitted that it was taken from the wound by the lymphatic vessels of the parts with which it was in contact; that after being absorbed, it was carried by these vessels towards the glands of the groin; that after traversing these bodies, it was conveyed, still by the lymphatic vessels, to the thoracic duct; finally, that it was introduced into the sanguiferous system by the communications which the thoracic duct preserves with the subclavian veins, and principally with those of the left side.

Such ought to have been, and such, in fact, was our opinion, at the time of the publication of the memoir upon the strychnos. Nor were the experiments, of which I am about to give an account, undertaken with a view to discover new facts, but rather to add a degree of certainty to an explanation already admitted; and our labours did not take a particular direction, until a greater number of facts obliged us to modify our view of this subject. But so rapid an absorption by vessels, whose principal characteristics are weakness and slowness of action; a poisonous substance that so quickly pervades the difficult and winding route of the lymphatic glands, without any alteration therein, were two circumstances that ought, perhaps, to have made us entertain some doubts of the correctness of the received explanation. This explanation, however, is given by so many respectable persons, and is supported by experiments so positive, that even now, when we have many facts to oppose it, we dare not say that it wants exactness, but only, that it is not admissible under every circumstance.

Previous to any detail of our experiments, it will not be useless to relate, in a few words, an opinion which for some time balanced the present prevailing belief relative to the organs of absorption.

This opinion, professed by Boerhaave, Haller, Méckel, Ruysch, Swammerdam, and others, was, that the sanguiferous veins, in common with the lymphatic vessels, possessed an absorbing power.

It is supported by different circumstances of structure, and by some physiological and pathological facts. A series of interesting experiments, undertaken and executed a few years since, at the veterinary school of Alfort, has also strengthened the

probability of such absorbent property of the veins, but without producing entire conviction. It is well understood, that an opinion established upon the physical structure of the organs, deduced from a sufficient and conclusive number of experiments, and supported by the names of Boerhaave, Haller, and Ruysch, ought not to be easily abandoned. Nor was less required than the anatomical discoveries of the last century, the correct experiments of Hunter and his brother, those of Cruikshank, Mascagni, Desgenettes, and others, to establish the belief that the lymphatic vessels only possess the absorbent faculty.

I will further cite, in support of the general opinion, some very curious experiments lately made by M. Dupuytren. This physiologist, who has kindly permitted me to report the principal results of his labours, tied the thoracic duct in several horses; some of them died in five or six days, others preserved every appearance of health. We know already, by an experiment of Duverney, by some observations on the thoracic duct when obstructed, and above all, by the experiments of Landrin, that the thoracic duct may cease to convey the chyle into the subclavian vein, without being followed by the death of the animal; it is true, we also know, that some animals died in consequence of a ligature round the duct; but we are entirely ignorant of the cause of this diversity in the results. M. Dupuytren, by his experiments, has found one very satisfactory. In the animals that died in five or six days, from the ligature round the thoracic duct, he always found it impossible to pass any injection from the inferior part of the duct into the subclavian vein; consequently, it is very probable, that the chyle ceased to be conveyed into the venous system immediately after the application of the ligature. On the contrary, in all the animals who had survived the application of the ligature, it has always been easy to make every kind of liquid pass from the inferior part of the duct to the subclavian veins, by means of the very numerous communications between these two points by the lymphatic vessels, placed alike in the posterior as in the anterior mediastinum.

I have personally assisted M. Dupuytren in opening a horse, the thoracic duct of which he had tied more than six weeks before, and I easily satisfied myself that there existed evident

communications between the inferior portion of the duct and the subclavian veins, although this canal was entirely destroyed at the place of the ligature.

I now proceed to the experiments, which I made, for the most part, in concert with M. Delile, to determine, whether the lymphatic vessels are really the only ones by which foreign substances enter into the venous system.

A circumstance that has always thrown some obscurity over the experiments upon absorption, is, the difficulty of demonstrating, with certainty, the passage and presence of absorbed matters, either in the lymphatic or sanguiferous vessels. We have not these inconveniences to fear in employing the upas, or the *nux vomica;* for it is known, that two centigrammes of these substances produce effects too remarkable to be mistaken.

Would the ligature round the thoracic duct stop the passage of the poison in the sanguiferous system, and consequently, its effects upon the spinal marrow? This was the first question that we proposed to resolve.

I have applied in a dog a ligature to the thoracic duct, a little before its opening into the left subclavian vein; I afterwards introduced a solution of the *upas* into the cavity of the peritoneum. The effects of the poison were as prompt and marked as if the thoracic canal had not been tied. I have tried the same ligature in other animals; but instead of introducing the poison into the cavity of the peritoneum, I introduced it either into the pleura, or into the stomach, the intestines, or muscles of the thighs, &c. The effects have always been equally rapid and intense, as if the thoracic canal had been free.

Decisive conclusions cannot be drawn from these first essays, for we know, that the thoracic canal is not the only point of communication between the lymphatic and venous systems. There is ordinarily, on the right side, a second thoracic duct, almost as considerable as that on the left; the large lymphatic vessels often open themselves solitarily into the subclavian veins; and more frequently still the thoracic duct has several mouths into the veins where it terminates.

One of these circumstances was found to occur in the animals subjected to our experiments; and we must have recourse to other trials, from which other results, less equivocal, may be deduced.

Upon a dog who had eaten a large quantity of meat seven hours before, in order that his abdominal lymphatic vessels might be easily perceived, we made an incision through the parietes of the abdomen, and took from thence a coil of the small intestines, upon which we applied two ligatures about five inches apart from each other. The lymphatics, which had their origin in this turn of the intestine, were very white and very apparent from the chyle which they contained. Two ligatures were placed at the distance of one centimetre upon each of these lymphatics; we cut these vessels between the two ligatures; we took great care in our experiment, and assured ourselves, by every possible means, that the curvature of the intestine, taken from the abdomen, had no further communication with the body by the lymphatic vessels. Five arteries and five mesenteric veins ended in the portion of the intestine comprised between the two ligatures; four of these arteries and four of the veins were tied and cut in the same manner as the lymphatics; the two extremities of the curvature of the intestine were cut and entirely separated from the rest of the small intestine. Thus we had a portion of the intestine about five inches in length, not communicating with the rest of the body, but by one artery and one mesenteric vein; these two vessels were separated at the distance of four fingers breadth. We even raised the cellular substance, lest some lymphatic vessels might lie there concealed. We had nothing further, by which to obtain a positive result, than to inject a small quantity of the *upas tieuté* into the cavity of the intestinal curvature. This was also done with proper precautions to prevent the escape of the injected liquor. The curvature of the intestine, enveloped in fine linen, was replaced in the abdomen; it was then precisely one o'clock. To our great astonishment, about six minutes after, the general effects of the poison diffused themselves with their usual intensity, and in such a manner, that every thing proceeded as if the curvature of the intestine had been in its natural state.

The animal being dead, we examined the parts; no ligature was displaced, nothing could make us suppose that the poison had passed into the abdominal cavity.

This experiment, repeated several times, without any modification in the result, appeared to us most conclusive; it proves,

at least, as far as can be proved in physiology, that the lacteal vessels are not the exclusive organs of intestinal absorption.

This kind of absorption, different from that of the lymphatic, may be peculiar to the intestines; it would be important to know, if it could be discovered to exist in the other parts.

We separated from its body, the thigh of a dog, previously rendered comatose by opium, (in order to spare it the pain of a difficult experiment;) this separation was so made, that the thigh still communicated with the trunk by the *crural* artery and vein. We took, with regard to these two vessels, the same precautions as for the mesenteric vein and artery in the preceding experiments; that is, we isolated them on an extent of four centimetres, and raised their cellular *coat*, lest it should conceal some lymphatic vessels; we then inserted two grains of the poison in the foot, and waited the effects. They manifested themselves with as much promptitude and energy, as if the thigh had not been separated from the body; insomuch, that the first signs of the action of the upas appeared before the fourth minute, and the animal died before the tenth.

It may be objected, that notwithstanding all the precautions taken, the arterial and venous parietes still contained some lymphatics, and that these vessels were sufficient to give passage to the poison. It were easy to refute this objection.

I repeated, upon another dog, the preceding experiments, with this modification, that I introduced in the crural artery a small quill, upon which I fixed this vessel by two ligatures; the artery was afterwards cut circularly between these two bands. I did the same to the crural vein; so that there was no longer any communication between the thigh and the rest of the body, unless by the arterial blood which comes to the thigh, and by the venous blood which returns to the trunk. The poison introduced into the foot produced its general effects in the ordinary time, that is, in about four minutes. It may be deduced, I believe, from these different experiments, that the lymphatic system is not, at least in certain cases, the exclusive route that foreign substances take to arrive at the venous system.

This new mode of absorption, much more direct than that by the lymphatics, presents the means of easily conceiving the

rapidity with which the different deleterious and other matters are absorbed, as well as the rapidity with which they produce their effects upon the system.

But what are the organs that first absorb the poison from the parts where it has been introduced? Are they the radicles of the veins, or are they rather the capillary lymphatics, which, having immediate anastomosis with the sanguiferous capillaries or exhalants, would immediately diffuse the poison through the venous system?

The experiments which I have just related, joined to those which have been made on the same subject, appear to me totally insufficient to decide either of these questions; only, it ought to be remarked, that our experiments are strongly in favour of a direct absorption by the veins.

But it is a fact, rendered evident by the preceding experiments, and upon which it is necessary to pause a moment, that the venous blood becomes charged with the poison; and that, by the intervention or means of this blood, the poison produces its deleterious action upon the system. In fact, if in the experiments where I had separated the thigh from the trunk, we suspend the course of the venous blood, by compressing between two fingers the crural vein, we lessen, and even totally suspend, the production of the effects. The blood of an animal, in which the signs of action of the *upas* is developed, contains then, some portion of poisonous matter; indeed, it may be said to be really poisoned. It were curious and interesting to know, if this blood, carried into the circulatory system of a healthy animal, would produce effects similar to those it had upon the animal itself. At first sight, we are led to believe that this is extremely probable, even that it is certain. The following experiments will shew with what care we ought, in physiology, to distinguish that which is probable from that which is proved by experiment.

We passed the arterial blood of an animal, in which the tetanus caused by the upas was manifest, into the jugular vein of a healthy animal; the transfusion lasted near twenty minutes, so that the healthy animal received a very considerable quantity of poisoned blood, which at the first moment of the experiment, was of a red and vermilion colour, and which after-

wards became violet and black, when the upas had produced asphyxia. There did not, however, appear any irritation of the spinal marrow, and the animal only experienced what happens in common transfusions, made with care. I mean, that it had, for some hours, a very marked acceleration of the inspiratory and expiratory motions as well as a very abundant pulmonary exhalation. Frequently repeated, this experiment has always produced the same results.

We were now certain, that the arterial blood of animals, poisoned by the *upas tieuté*, the *nux vomica*, or the *bean* of *St. Ignatius*, was not susceptible of producing similar effects on other animals; it would not be, perhaps, the same with the venous blood. It may be presumed, that the respiratory action changed the nature of the poisonous substance; and this alteration might, to a certain degree, give the reason, why the transfusion of the arterial blood of animals poisoned by the strychnos, is not followed with bad effects.

This circumstance did not take place with the venous blood, which returns from the part where the poison has been introduced. After the experiments reported in the memoir upon the upas, and in this, it is impossible to doubt, that this blood does not transport the poison to the lungs. It is very probable, that introduced into the circulatory system of another animal, it would produce effects similar to those which it caused upon the animal on which the inoculation of the poison was made.

A small piece of wood covered with two grains of *upas tieuté*, was struck into the thick part of the left side of the nose of a dog. Three minutes after this introduction, we passed into the venous system of another dog, the blood of the jugular vein of the side where the introduction of the poison had been made. The transfusion commenced about one minute before the first signs of the upas; it did not cease until the death of the animal who experienced it. No appearance of irritation of the spinal marrow was perceived in the animal who received so great a quantity of poisoned blood.

Although these experiments were repeated several times, with variations in the mode of introducing the poison, we never could perceive in the healthy animal, who had suffered the

transfusions of poisoned blood, any thing which resembled the effects of the strychnos.

Results so positive, appear to us of a nature to warrant the conclusion, that the venous blood of animals poisoned by the *upas*, the *nux vomica*, and the *bean* of *St. Ignatius*, is no more capable than the arterial blood, of producing upon another animal the effects which it will cause upon the animal from which it was taken.

If there still remained any doubts, they would be removed by the following experiment, which was repeated several times.

As in the experiments above related, we separated, from the body, the thigh of an animal, isolating as before the crural artery and vein; we introduced the poison into the separated foot, and transfused the blood of the crural vein into the jugular vein of a sound animal. The passage of the blood from one animal to the other, lasted more than ten minutes, a time more than sufficient for the production of the effects of the upas. But no sign of the action of this poison was perceived, either in the one or in the other animal. The one preserved perfect health, the other died in a few days, in consequence of the amputation of the thigh, and the loss of blood which was transfused.

It must not, however, be thought, that in this experiment the transfused blood, by some particular cause, had no deleterious properties, for the following experiment proves the contrary.

As in the preceding experiment, I separated the thigh from the body; three minutes after introducing the poison into the foot, I passed the blood of the crural vein into the jugular vein of another animal; the transfusion was prolonged five minutes without producing any effects. I then stopped it, and disposed things in such a manner that the blood of the crural vein should return to the animal to which it belonged. Almost instantly this animal exhibited evident signs of the action of strychnos on the spinal marrow.

From the different experiments reported in this memoir, we must, I think, conclude,

1st. That the lymphatic vessels are not always the route followed by foreign matters, to arrive at the sanguiferous system.

2. That the blood of animals, upon which the bitter strychnos has produced its deleterious effect, cannot produce any fatal effects upon other animals.

As to the explanation of this singular phenomenon, it would be, I think, premature to give it at present. In physiological science, we ought to be sparing of conjectures, and prodigal of facts.

MAGENDIE, D. M. P.

Note.—This Memoir has received the approbation of the Institute.

On Painful Affections of the Side from Tumid Spleen.

By ROBERT BREE, M. D. F. R. S.

[From the Medico-Chirurgical Transactions, Vol. II. No. 1.]

Amongst the painful affections of the side that may be attributed to internal causes, the disorders of the spleen are distinguished with difficulty in their early stage.

The histories of morbid spleen which have been given by anatomists, do not furnish a satisfactory account of the early signs of the disease. The symptoms which recently preceded death, are often minutely detailed; but as these are necessarily connected with the states of other viscera which had suffered inflammation, the case is deprived of its distinct character, and the description shews the result of complicated disorders, but explains little of their progress, and still less of the beginning of a simple and limited complaint.

On the state of the spleen itself, they report certain appearances which must have proceeded from disease, and not from original defect of structure.

The spleen has been wasted to the smallest size. The capsule only containing a little of its reticular or vascular substance, and possessing its peritoneal covering.

A second state is the excessive enlargement of the spleen. The natural weight of this organ may vary from 9 ounces to 14 ounces; but after chronic diseases of the viscera, and intermit-

tent fevers, it has weighed from one pound to 20, and even 30 pounds.

A third state is that of its structure being destroyed, and its substance corrupted or broken down into a sanious mass, which has often distended, and sometimes ruptured the coats. This condition has been the sequel of long protracted disease, or the effect of fevers of a rapid and particular character.

Fourthly, a great portion of the substance of the spleen has been found in a cartilaginous state, and occasionally its peritoneal coat ossified.

Systems of medicine do not afford us more satisfaction by their descriptions of Splenitis: Cullen says (ccccxxv) that it may be readily known by his definition in the Nosology: he refers also to his doctrine on the inflammation of the other abdominal viscera for the practice which it requires, as well as for information respecting its three modes of termination, by " resolution, suppuration, or gangrene."

His account of Splenitis agrees with that of writers in general, but it corresponds so little with the facts which actually appear in the early stages of the disease, and the practice is so insufficient or inapplicable at this period, that it is surprising the defect should hitherto have escaped observation.

Splenitis, or more properly speaking, " the tumid and painful spleen," is not at its commencement a febrile disease; and yet this affection is so sensibly felt, that the character by which *chronic* Hepatitis is distinguished from *acute*, cannot be applied to it. Whilst inflammatory or febrile action is absent, the disease cannot terminate by " suppuration or gangrene." Nor can it, for the same reason, end by " resolution," if this term be taken in its ordinary meaning.

It will appear that the turgid condition of this viscus may continue for many months, during which time the pain may be acute, and the swelling may be perceptible under the spurious ribs, and no fever may attend the complaint.

An increase of pulse takes place, as recovery from the actual disease proceeds, so that it not only might exist without fever, but a moderate degree of fever may be an indication of the return of health to the organ.

It must be unnecessary to speak here of the structure of the

spleen, after the account which has been given of this viscus by CUVIER, and confirmed by the recent investigation of Mr. HOME.

The parenchyma of this viscus, however little it may be disposed to inflammation, is susceptible of injury from the distension of its numerous vessels, which may be so great as to overcome their power of contraction, at the same time that the ligamentous fibres may be weakened in an equal degree, or wholly lose their elasticity. Inflammation may be expected to supervene on this condition at some period; but I believe this never takes place, but on the accession of injury to the peritoneal coat, either from the internal distension having been of long standing, or from external violence.

Having had favorable opportunities of considering abdominal diseases complicated with asthmatic affections, I have been led to pursue a treatment which I have thought more appropriate to the nature of splenic disorders, than what I had been directed to by books, or had observed in general practice.

I had noticed, in a protracted case of dyspnœa, with pain of the left side, the manifest relief that was obtained from a free discharge of hæmorrhoids, and I endeavoured to imitate this process of nature. The patient was a young man of 22 years of age. He had suffered attacks of tertian fever two years before, and had been in feeble health, with constant uneasiness of the left side, during the last year. The ague had left him when he changed his residence a year and an half preceding my attendance; but the pain of his side had resisted all the means that had been applied for his relief. At length the piles bled considerably for the first time, and he was restored to his former ease for three months. On the approach of winter, the disease returned and gradually became as oppressive as before. He was unable to rest on his right side. There was no external sign of enlargement on the left side; but he had a sense of weight and fulness, with severe pain under the short ribs, extending to the lowest rib on that side. There was no fever, and the state of the belly and urine was natural. I proceeded to treat the case with aloetic aperients united with assafetida and sulphate of potash. In a few weeks the piles returned and carried off the complaint. As I had considered this pain of the

side as an affection of the spleen, I placed the result of the case amongst the facts that influenced my opinion on similar disorders, which afterwards came under my care. Many of these were cases of female patients, in whom dyspnœa and pain in the region of the spleen, were wholly removed or greatly relieved by the flow of the menses.

I was directed by this experience, casually acquired, to the practice which I have since pursued in diseases of the spleen.

I beg to submit to the consideration of the society the following case, as explanatory of the remote cause of turgid and painful spleen, and of the manner of treating this affection with success.

CASE.

M. A. October 26, 1809.

This young lady is 17 years old, of a fair complexion, and of much vivacity and intelligence of mind. She has pain of the left side, which began a month ago, and has gradually increased to the present time. She is unable to lie on the right side, as this posture aggravates the pain of her left side. She has no expectoration, but occasionally a very trifling short cough.—Pulse is 62, and soft. Upon examining the side, no difference is perceived by the eye or hand, but she feels pain under the short ribs upon pressure, and the pain descends lower than the ribs. She is also sensible of bulk and weight under these ribs, which extend from the margin of the eighth rib to the lowest rib, and backwards to the spine. She was at school when this complaint was first felt in a slight manner. She had a puke and dose of calomel given to her without relief; but there was no particular examination of the part at this time. The pain afterwards became gradually connected with giddiness of the head, and difficulty of breathing, particularly when the body was put in motion. In this state she was brought to her friends in town. Two months before this date, and one month before she complained of local pain, she had been bathing in the sea, with some young friends of her family; and she plunged herself 62 times into the water, in a playful humor and in opposition to the advice of the elder part of the company. Though she does not allow that she received immediate injury from this act, it

had been considered by her friends as injurious, and it was always referred to by them as the cause of her complaint.

The symptoms I have enumerated, appeared to indicate congestion in the spleen; nor could I account for the slowness of the pulse, accompanied as it was with fixed and acute pain in the region of the spleen, and difficulty of breathing, by any other probable cause. I prescribed only saline draughts with opium every six hours, and an aperient every morning, at this visit.

October 31st.—I had seen Miss A. several times. I found little variation in the symptoms. A small qnantity of wine had aggravated her complaints of vertigo and local pain. The pulse was never more frequent than 62, and generally at 60. There was no heat of the skin, and no sickness attended her vertigo. The tongue was clean, and the discharges of stool and urine natural. There was no cough. But her nights were very much disturbed by the difficulty of lying easy in any posture, and particularly on her sides, the pain of the left side being insupportable when she turned to her right side. The plan of medicine had not been varied; but she had been bled largely without relief. The blood was not inflamed, but so loose in its texture as to show the operation to have been useless, if not improper. Blisters had been applied with as little advantage. She was now directed to take a pill of calomel and opium every six hours with her saline draught, and the purging draught every morning made more active by tincture of jalap and aloes.

To abate pain, pulv. ipecac. compos. was directed to be taken in sufficient doses at bed time.

She proceeded on this plan for several days, during which time her pulse did not rise higher than 64, and generally was found at 60. The urine was increased in quantity, and the skin was more disposed to perspire. She had two loose stools every day. She yet experienced no ease in any posture in her bed excepting on the back.

Nov. 17.—In the first week of November, Miss A. appeared to have some relief; but none of her complaints were removed; and in addition to them she had experienced palpitation of the heart, which also interrupted her repose. She complained in general of more languor than she had been used to feel before

she was bled. She had had five grains of extract of conium twice a day in her pills for the last week: and she took three days since an active vomit, which occasioned the discharge of a large mass of mucus, so considerable as to surprise the observers. There was no bile thrown off, nor any thing that had a particular taste. No benefit or change of feeling resulted from this operation, excepting that she found herself lighter at the præcordia. The vertigo at this time was so distressing that she could not walk across the room without assistance to prevent her from falling.

I now determined to make trial of a plan, which might strengthen and excite the system. I prescribed 15 grains of myrrh with carbonated ammonia and camphor mixture to be taken every six hours. The former medicines were to be discontinued, excepting such proportions of opium and extract of conium, as might be necessary to abate pain.

Nov. 24.—She perceived some relief of her languor, but no mitigation of the other symptoms. Three grains of sulphate of iron were now prescribed in a draught, with myrrh and camphor, to be taken every six hours; and the bowels were to be kept open by the usual means. She was also advised to try the effect of gentle exercise on horseback.

Dec. 10.—Miss A. had not yet rode on horseback. The tonic plan had been frequently varied. She had taken bitters—decoct. cinchonæ—infus. cascarillæ with rhubarb—extract of hop and sulphate of zinc, besides the forms before reported. The pulse had remained during this course at the average of 60, and had therefore been something less frequent than I had generally observed it to be, during the course of saline aperients with calomel. She now made one trial of horse exercise at a gentle walk, but was obliged to desist in half an hour, from the increase of pain of the side, which gradually became insupportable. After this she considered her complaint worse, which she attributed to the riding; but it was evident that the tonic plan had a full share in producing the aggravation of her complaint. I examined her side, where she assured me she felt much increase of bulk, and I perceived a protrusion of substance from within to the margin of the false ribs. This swelling was so manifest on several examinations, that my attention

was recalled to the treatment which I had applied in other cases of a similar nature, and which had been partly pursued in this with more success than the last which I had directed.

Dec. 15.—I advised her to remain constantly in her chamber, that she might pursue without deviation a course of medicines, by which the secretions were to be much increased, and determined chiefly to the bowels and kidneys, whilst the body was in a state of rest. She was willing to acquiesce in all my directions, and I resolved to act upon this principle for a considerable length of time. A saline draught was ordered to be given every six hours, with one grain of aloes and five grains of extract of conium.

Dec. 26.—Having pursued this plan during ten days without variation, excepting in the degree of the strength of the purgative, the symptoms were reduced to the state which prevailed in the last month. The saline aloetic draughts had produced several motions daily without lowness. The medicines were continued with 15 grains of nitre in each draught.

Jan. 9, 1810.—At the beginning of this month, Miss A. was sensible of considerable relief from her complaint; but she was still unable to lie upon the right side. The urine was copious, and perspiration free every night. She had five or six motions daily without any sense of languor; and this state of the bowels seemed to conduce most materially to her improvement. She was recommended to take two grains of antimonial powder, and two grains of aloes three times a day, with gr. iv. of extract of conium. A saline draught with nitre and camphorated tinct. of opium was to be taken every night at bed time.

Feb. 1.—Upon an examination and pressure of the side at the end of January, she was found to be much less sensible of uneasiness, and there was no appearance of swelling. But she still suffered too much to persist in any attempt at lying on the right side. Her dyspnœa was less, she could move without giddiness, and she was much improved in flesh and colour, notwithstanding her close confinement. The pulse had risen to the average of 75. She was directed to continue the medicines with the addition of four grains of extractum papaveris at bed time.

Feb. 5.—The bowels were more torpid than before she took the extr. papav. which, as it had not produced more rest in the night, was discontinued. A mixture of infusion of senna with neutral salts was directed to be taken every morning, and the aloetic antimonial pills to be continued.

In the middle of February she was so far improved, as to propose to leave her chamber. She had insensibly come to lie on the right side. And her complaints were so little felt, as to be spoken of with indifference. The pulse was 80.

March 1.—Miss A. had been a sufferer with her disease above five months; and she had lived in her chamber nearly three months. The catamenia had been regular, and always relieved her complaints. She now considered herself well, as her nights were good, and she very seldom felt pain in her side, difficulty of breathing, or giddiness, and she could use exercise without inconvenience. She was free from all her complaints in the middle of the month; and at the end of March she performed a long journey without suffering any relapse. At the present time she is perfectly well, and much fatter than she was before her illness.

It would be intruding upon the time of the society, if I were to go into a comparison of this case with others, in which congestion and inflammation of the spleen had been proved by examination after death. Its identity has been confirmed to me by the observation of many cases that had been previously under my care. And I believe that the more advanced stages of splenitis, in which inflammation has affected the membranous coverings and connected viscera, may be often followed up to this simple state of disease, as the root from which such complicated disorder had grown.

This and other instances of similar affections, have suggested to me some inferences, which I now offer with deference to the consideration of the society.

This disease appeared to be the direct effect of a repressed circulation from the application of cold to the surface of the body, and every consideration of the nature of such a cause leads to the conclusion, that congestion had taken place in the spleen.

Whatever degree of pain was suffered, it did not increase

the frequency of the pulse, which remained at a slower rate than was natural in the state of health. I consider this state of the pulse as diagnostic of the turgid state of the spleen, when it is accompanied by the symptoms of great pain, and of inability of lying on the contrary side; in which position, the heavy and enlarged spleen acts as a dead weight, drawing the diaphragm, and distressing the adjoining parts. The absence of fever may be explained by the nature of the structure of this organ. It must have great capacity in its arteries when they are dilated to receive blood, that is impelled into them: and it suffers distension for the safety of the system, pyrexia being prevented by the actual turgescence of all its vessels. It is more than probable that there may be pains of the side from turgescence of the spleen, of so obtuse or mild a nature, as to be supported without much complaint. Relief from these pains may be obtained by determining the blood to the lower belly and pelvis: and they are to be partially removed from women by the increase of the catamenia, and from men by the bleeding of the hæmorrhoidal vessels. When there is much dyspnœa, without fever, attending a heavy uneasiness in the left side, the spleen may be considered as the suffering part, even if there be no external tumor. This conclusion will be stronger, if there be giddiness of the head and distress from lying on the right side. We may presume, that though the internal structure of the spleen is loaded with blood, the capsule and the peritoneal coat are not yet distended, or not in a considerable degree.

From this inferior degree of affection, a more formidable complaint must grow, if it be neglected, or treated erroneously.

The integuments are next distended, and the pain increases in greater proportion than the distension. The diaphragm is spasmodically affected, and the uneasiness is carried under the sternum to the opposite side, while the dyspnœa takes on the character of convulsive asthma. The vertigo, upon motion of the body, increases with all other symptoms. There is no fever even yet, and therefore, notwithstanding such an extent of suffering, there is no inflammation.

I consider this collection of symptoms to make the first stage of splenitis, or turgid and painful spleen.

With respect to the treatment of this disease, the rules for

its removal are few and limited. This will not appear extraordinary, if we attend to its structure and its œconomy, which seem to require a peculiar practice.

The blood must not be driven into it by stimulants during its state of debility: it must then be allowed to rest, that the energy of its vascular and ligamentous substance may have time to recruit. The œconomy of the spleen will, in every respect, make this process slow; but during its passive state, the hæmorrhoidal arteries may be excited to receive an unusual portion of the circulating mass, and to discharge it. The excretories of the kidneys, and the exhalants of the intestines, and the hæmorrhoidal vessels may all produce relief to the spleen when their action is increased. I have not found issues, blisters, or any other external applications produce the slightest advantage.

Bleeding may be supposed to be useful, as it lowers the impulse of the blood to the organ affected, giving its vessels time to exert themselves by rest. Yet this means has not answered well, possibly because it may lower the general tension without promoting that fulness of the vessels going downwards, which is necessary to recovery.

Bleeding by leeches, as well as blisters, must be necessary in the advanced stages, where the peritoneal coat is inflamed, and adhesions have taken place; but we are not treating of this part of the progress of the splenic disease.

Mercury has appeared to be injurious, excepting as it may be directed to the purpose of purging the intestines and opening their exhalants.

Chalybeate tonics might be expected to be eminently serviceable in the passive state of the splenic vessels; but in this expectation we are disappointed, unless they be given after these vessels and the ligamentous part of the spleen have recovered some degree of contractile power. If the circulation be hurried before this improvement has taken place, or if it be excited at any period of the complaint in an undue degree, the turgescence will be augmented in proportion to the action of the heart and arteries.

If there be any exception to this remark, it must apply in

very depressed states of the habit, the consequence of long protracted illness.

Whenever the spleen is turgid, the blood should be diverted from finding its course to this organ; which is most likely to be effected by determining it to the other vessels of the abdomen and pelvis. In attempting to produce this necessary change, and in the intention of cure, aloës with antimonials and neutral salts have appeared to me to be the most successful instruments, when used twice in the day at least with great perseverance, and not as cathartics are generally prescribed, at intervals longer than twenty-four hours.

Chemical Researches on the Blood, and some other Animal Fluids. By William Thomas Brande, *Esq. F.R.S. Communicated to the Society for the Improvement of Animal Chemistry, and by them to the Royal Society.*

[From the Philosophical Magazine, for August 1812.]

Section I.

Introduction.

In the following pages I shall have the honour of laying before this society an account of some experiments upon the blood, which were originally undertaken with a view to ascertain the nature of its colouring matter. The difficulties attendant on the analysis of animal substances have rendered some of the results less decisive than I could have wished, but I trust that the general conclusions to which they lead, will be deemed of sufficient importance to occupy the time of this body.

The existence of iron in the blood was first noticed by Menghini*, and its peculiar red colour has been more recently attributed to a combination of that metal with phosphoric acid, by MM. Fourcroy and Vauquelin†. The very slight discoloration occasioned by the addition of infusion of galls to a solu-

* Vincentius Menghinus de Ferrearum Particularum Progressu in Sanguinem. *Comment. Acad. Bonon.* t. ii. p. 2, pag. 475.

† *Systême des Conn. Chym.* vol. viii.

tion of the colouring matter, under circumstances most favourable to the action of that delicate test of iron, first led me to doubt the inferences of those able chemists, and subsequent experiments upon the combinations to which they allude, tended to confirm my suspicion, and induced me to give up no inconsiderable portion of the time which has elapsed since the last meeting of this Society, to the present investigation.

An examination of the chyle and of lymph, in order to compare their composition with that of the blood, formed an important part of this inquiry, especially as those fluids have not hitherto been submitted to any accurate analysis, on account of the difficulty of procuring them in sufficient quantities, and in a state of purity. Whilst engaged in assisting Mr. Home in his physiological researches, several opportunities occurred of collecting the contents of the thoracic duct under various circumstances, and in different animals; on other occasions Mr. Brodie has kindly furnished me with the materials for experiment.

Section II.

On the composition of Chyle.

The contents of the thoracic duct are subject to much variation. About four hours after an animal has taken food, provided digestion has not been interrupted, the fluid in the duct may be regarded as pure chyle; it is seen entering by the lacteals in considerable abundance, and is of an uniform whiteness throughout. At longer periods after a meal, the quantity of chyle begins to diminish, the appearance of the fluid in the duct is similar to that of milk and water; and lastly, where the animal has fasted for twenty-four hours or longer, the thoracic duct contains a transparent fluid which is pure lymph.

A. The chyle has the following properties.

1. When collected without any admixture of blood, it is an opake fluid of a perfectly white colour, without smell, and having a slightly salt taste, accompanied by a degree of sweetness.

2. The colour of litmus is not affected by it, nor that of paper stained with turmeric, but it slowly changes the blue colour of infusion of violets to green.

3. Its specific gravity is somewhat greater than that of water, but less than that of blood; this, however, is probably liable to much variation.

4. In about ten minutes after it is removed from the duct, it assumes the appearance of a stiff jelly, which in the course of twenty-four hours gradually separates into two parts, producing a firm and contracted coagulum, surrounded by a transparent colourless fluid. These spontaneous changes, which I have observed in every instance where the chyle was examined at a proper period after taking food, are very similar to the coagulation of the blood and its subsequent separation into serum and crassamentum; they are also retarded and accelerated by similar means.

B. 1. The coagulated portion bears a nearer resemblance to the caseous part of milk than to the fibrine of the blood.

2. It is rapidly dissolved by the caustic and subcarbonated alkalies. With solutions of potash and soda, it forms pale brown compounds, from which, when recent, a little ammonia is evolved. In liquid ammonia the solution is of a reddish hue.

3. The action of the acids upon these different compounds is attended with nearly similar phenomena, a substance being separated intermediate in its properties between fat and albumen. Nitric acid added in excess redissolves this precipitate in the cold, and sulphuric, muriatic, and acetic acids when boiled upon it for a short time.

4. Neither alcohol nor ether exerts any action upon the coagulum of chyle; but of the precipitate from its alkaline solution they dissolve a small portion, which has the properties of spermaceti: the remainder is coagulated albumen.

5. Sulphuric acid very readily dissolves this coagulum, even when diluted with its weight of water; and with the assistance of heat, it is soluble in a mixture of one part by weight of acid, with four of water; but when the proportion of water is increased to six parts, the dilute acid exerts no action upon it. I was surprised to find that the alkalies produced no precipitation in these sulphuric solutions when heat had been employed in their formation, and where a small proportion only of the coagulum had been dissolved, and was therefore led to examine more particularly the changes which the coagulum had undergone by the action of the acid.

On evaporating a solution of one drachm of the coagulum in two ounces of dilute sulphuric acid (consisting of one part by weight of acid with three of water) down to one ounce, a small quantity of carbonaceous matter separated, and the solution had the following properties.

It was transparent, and of a pale brown colour.

Neither the caustic nor carbonated alkalies produced in it any precipitation, when added to exact saturation of the acid, or in excess.

Infusion of galls, and other solutions containing tannin, rendered the acid solution turbid, and produced a more copious precipitation in that which had been neutralized by the addition of alkalies.

When evaporated to dryness, carbonaceous matter was deposited, and sulphurous acid evolved, with the other usual products of these decompositions.

6. On digesting the coagulum in dilute nitric acid, consisting of one part by weight of the acid to fifteen of water, it was speedily rendered of a deep brown colour, but no other apparent change was produced for some weeks; when on removing it from the acid at the end of that period, it had acquired the properties of that modification of fat which is described by Fourcroy under the name of adepocire.*

A mixture of one part of nitric acid with three of water, acted more rapidly upon the coagulum of chyle; a portion of it was dissolved, and when the acid was carefully decanted from the remainder, it was found to possess the properties of gelatine. But when heat was applied, or when a stronger acid was employed, the action became more violent, nitrogen and nitric oxide gas were evolved, and a portion of carbonic acid and of oxalic acid were produced.

7. Muriatic acid in its undiluted state does not dissolve the coagulum of chyle; but when mixed with an equal quantity of water, or even more largely diluted, it dissolves it with facility, forming a straw-coloured solution, which is rendered turbid when the alkalies are added to exact saturation, but no precipitate falls, nor can any be collected by filtration. When either

* *Mem. de l'Acad. des Sciences*, 1789.

acid or alkali is in excess in this solution, it remains transparent.

8. Acetic acid dissolves a small portion of the coagulum of chyle, when boiled upon it for some hours. As the solution cools, it deposits white flakes, which have the properties of coagulated albumen.

9. The action of oxalic acid is nearly similar to that of the acetic, but neither citric nor tartaric acid exert any action upon this coagulum.

10. The destructive distillation of this substance affords water slightly impregnated with carbonate of ammonia, a small quantity of thin fetid oil and carbonic acid and carburetted hydrogen gas.

The coal which remains in the retort is of difficult incineration; it contains a considerable portion of muriate of soda and of phosphate of lime, and yields very slight traces of iron.

C. 1. The serous part of the chyle becomes slightly turbid when heated, and deposits flakes of albumen.

2. If after the separation of this substance the fluid be evaporated to half its original bulk, at a temperature not exceeding 200° Fahrenheit, small crystals separate on cooling, which, as far as I have been able to ascertain, bear a strong resemblance to sugar of milk: they require for solution about four parts of boiling water, and from sixteen to twenty parts of water of the temperature of 60°. They are sparingly soluble in boiling alcohol, but again deposited as the solution cools. At common temperatures alcohol exerts no action upon them. The taste of their aqueous solution is extremely sweet. By nitric acid they are converted into a white powder of very small solubility, and having the properties of saccholactic acid, as described by Scheele.*

The form of the crystals I could not accurately ascertain even with the help of considerable magnifiers. In one instance they appeared oblique six-sided prisms, but their terminations were indistinct.

Some of the crystals heated upon a piece of platina in the flame of a spirit lamp, fused, exhaled an odour similar to that

* Chemical Essays, No. xvii.

of sugar of milk, and burnt away without leaving the smallest perceptible residuum.

3. The destructive distillation of the serous part of chyle afforded a minute quantity of charcoal, with traces of phosphate of lime and of muriate of soda and carbonate of soda.

Section III.

Analysis of Lymph.

The fluid found in the thoracic duct of animals that have been kept for twenty-four hours without food, is perfectly transparent and colourless, and seems to differ in no respect from that which is contained in the lymphatic vessels. It may therefore be regarded as pure lymph.

It has the following properties*:

1. It is miscible in every proportion with water.

2. It produces no change in vegetable colours.

3. It is neither coagulated by heat, nor acids, nor alcohol, but is generally rendered slightly turbid by the last re-agent.

4. When evaporated to dryness, the residuum is very small in quantity, and slightly affects the colour of violet paper, changing it to green.

5. By incineration in a platina crucible the residuum is found to contain a minute portion of muriate of soda; but I could not discover in it the slightest indications of iron.

6. In the examination of this fluid, I availed myself with some advantage of those modes of electro-chemical analysis, which on a former occasion I have described to this Society.†

When the lymph was submitted to the electrical action of a battery consisting of twenty pairs of four-inch plates of copper and zinc, there was an evolution of alkaline matter at the negative surface, and portions of coagulated albumen were separated. As far as the small quantities on which I operated enabled me to ascertain, muriatic acid only was evolved at the positive surface.

* The term lymph has been applied indiscriminately to the tears, to the matter of encysted dropsy, and to some other animal fluids. Vide Aikin's Dictionary of Chemistry and Mineralogy, art. *Lymph.*

† Phil. Trans. 1809, p. 373.

Section IV.

Some Remarks on the Analysis of the Serum of Blood.

This fluid has been so frequently and fully examined by chemists, that I shall not enter into a detailed account of its composition, but merely state such circumstances respecting it as relate particularly to the present inquiry, and have not hitherto been noticed by the experimentalists to whom I have alluded.

The fluid which oozes from serum that has been coagulated by heat, and which by physiologists is termed *serosity*, is usually regarded as consisting of gelatine, with some uncombined soda, and minute portions of saline substances, such as muriate of soda and of potash, and phosphate of lime, and of ammonia. Dr. Bostock regards it as mucus.*

From some experiments which I made upon the serum of blood, on a former occasion, I was induced to regard the serosity as a compound of albumen with excess of alkali, and to consider the coagulation of the serum analogous to that of the white of egg, and of the other varieties of liquid albumen.

To ascertain this point, and to discover whether gelatine exists in the serum, I instituted the following experiments.

Two fluid ounces of pure serum were heated in a water bath until perfectly coagulated: the coagulum, cut into pieces, was digested for some hours in four fluid ounces of distilled water, which was afterwards separated by means of a filter.

The clear liquor reddened turmeric paper, and afforded a copious precipitation on the addition of infusion of galls, and when evaporated to half an ounce it gelatinized on cooling. It was rendered very slightly turbid by the addition of dilute sulphuric and muriatic acid; but alcohol produced no effect.

From the result of these trials, it might have been concluded that gelatine was taken up by the water; but as an akaline solution of albumen forms an imperfect jelly when duly concentrated, and as albumen and gelatine are both precipitated by tannin, I was inclined to put little reliance on the appearances just described, until I had examined the solution by the more accurate method of electrical decomposition.

* Transactions of the Medical and Chirurgical Society of London, vol. i. p. 73.

Upon placing it in the Voltaic circuit my suspicions were justified, by the rapid coagulation which took place in contact with the negative wire. I therefore made some other experiments in order to corroborate this result.

One fluid ounce of pure serum was dissolved in three of distilled water: the conductors from a battery of thirty pairs of four-inch plates were immersed in this solution at a distance of two inches from each other; the electrization was continued during three hours and a half, the solid albumen being occasionally removed: at the end of that period, no further coagulation took place, and a mere decomposition of the water was going on.

Having ascertained in previous researches, that gelatine is not altered during the electrical decomposition of its solution carried on as just described, my object in this experiment was, to ascertain whether any gelatine remained after the complete separation of the albumen had been effected. I accordingly examined the water from which the coagulated albumen had been removed, and found that it was not altered by infusion of galls, nor did it afford any gelatine when evaporated to dryness.

Two fluid ounces of dilute muriatic acid were added to one of serum. The mixture immediately assumed a gelatinous appearance; it was heated, and a more perfect coagulation of the albumen took place; the liquid part was separated by a filter. No effect was produced upon it by Voltaic electricity, nor did infusion of galls occasion any precipitation.

I repeated the first experiment with the addition of twenty drops of a solution of isinglass to the serum. The liquid which now separated, after the albumen had been entirely coagulated by the action of electricity, was copiously precipitated by infusion of galls.

It may be inferred from these experiments, that gelatine does not exist in the serum of the blood, and that the serosity consists of albumen in combination with a large proportion of alkali, which modifies the action of the re-agents commonly employed, but which is readily separated by electrical decomposition.

To ascertain whether iron exists in the serum of the blood, one pint was evaporated to dryness in a crucible, and gradually

reduced to a coal, which was incinerated and digested in muriatic acid, to which a few drops of nitric acid were added; some particles of charcoal remained undissolved; the solution was saturated with ammonia, which afforded a copious precipitation of phosphate of lime, accompanied with slight traces only of oxide of iron.

Section V.

Some Experiments upon the Coagulum of Blood.

Mr. Hatchett's valuable researches on the chemical constitution of the varieties of coagulated albumen, have shewn that that substance varies but little in its properties, whether obtained from the crassamentum of the blood, or from washed muscular fibre, or other sources; but that the proportion of earthy and saline matter is different in the different varietes.*

It will also be remarked, on referring to the dissertation which I have just quoted, that the ashes obtained by incinerating the coal left after the destructive distillation of albumen, did not contain any appreciable proportion of iron.

Assuming the existence of iron in the colouring matter of the blood, I made the following experiments upon the crassamentum of that fluid.

Two pints of blood were collected in separate vessels. The one portion was allowed to coagulate spontaneously; the other was stirred for half an hour with a piece of wood, so as to collect the coagulum, but to diffuse the principal part of the colouring matter through the serum. These two portions of coagulum were now dried in a water-bath, and equal weights of each reduced in a platina crucible to the state of coal, which afterwards was incinerated. The ashes were digested in dilute nitro-muriatic acid, and the solution saturated with liquid ammonia in order to precipitate the phosphate of lime, as well as any iron which might have been present.

The precipitates were collected, dried, and treated with dilute acetic acid, by which they were almost entirely dissolved, some very minute traces only of red oxide of iron remaining,

* Phil. Trans. 1800, p. 384.

the quantity of which was similar in both cases, and so small as nearly to have escaped observation.

It is reasonable to infer, if the colouring matter of the blood were constituted by iron in any state of combination, that a larger relative proportion of that metal would have been discoverable in the former than in the latter coagulum; but frequent repetitions of these experiments have shown that this is not the case, and the following result appears to complete the evidence on this subject.

The colouring matter of a pint of blood was diffused by agitation through the serum, from which it was allowed gradually to subside, the coagulum having been removed: after twenty-four hours the clear serum was decanted off, and the remainder, containing the colouring matter, after having been evaporated to dryness, was incinerated, and the ashes examined as in former experiments. But the traces of iron were here as indistinct as in the other instances above mentioned, although a considerable quantity of the colouring matter had been employed.

The minutiæ of analysis I have purposely excluded, as leading into details which would exceed the proper limits of this paper, and unnecessary in the present investigation. I shall now merely dwell on the principal results which have been obtained, and on the general conclusions which these afford.

Section VI.

Researches on the colouring Matter of the Blood.

1. To procure this substance for experiments, I generally employed venous blood which had been stirred during its coagulation: the fibrina is thus removed, and the colouring matter diffused through the serum, from which it gradually subsides, being difficultly soluble in that fluid; on decanting off the supernatant serum, the colouring matter remains in a very concentrated form. When other modes of procuring it were employed they will be particularly mentioned; but as I have not found the serum which is retained interfere much with the effects of various agents upon the colouring principle, the method just noticed was commonly adopted.

2. When the colouring matter thus collected is microscopi-

cally examined, it seems, as Lewenhoeck first observed,* to consist of minute globules. These are usually described as soluble in water, a circumstance which my own observations led me to doubt, and which the more accurate experiments of Dr. Young, an account of which, intended for publication, he has kindly permitted me to peruse, have completely disproved.

3. The effect of water upon the red globules, is to dissolve their colouring matter, the globule itself remaining colourless, and, according to Dr. Young, floating upon the surface.

This aqueous solution is of a bright red colour, and not very prone to putrefaction. When heated, it remains unaltered at temperatures below 190° or 200° Fahrenheit; at higher temperatures it becomes turbid, and deposits a pale brown sediment: if in this state it be poured upon a filter, the water passes through without colour, so that exposure to heat not only destroys the red tint, but renders the colouring matter insoluble in water.

Alcohol and sulphuric ether added to this solution also render it turbid, and when these mixtures were filtrated, a colourless and transparent liquor was obtained.

4. The matter remaining upon the filter was insoluble in water, in alcohol, and in sulphuric ether; but when digested in dilute muriatic or sulphuric acid, a portion was taken up forming a brown solution. I regard this soluble portion as a modification of the colouring matter produced by the operation of heat: the insoluble residuum had the properties of albumen.

5. Effects of Acids on the colouring Matter.

A. Muriatic acid poured upon the colouring matter of the blood, renders one portion of it nearly insoluble and of a bright brown colour: another portion is taken up by the acid forming a dark crimson solution when viewed by reflected light; but when examined by transmitted light, it has a greenish hue.

This solution remains transparent, and its colour is unimpaired by long exposure to light, either in contact with the air, or when kept in close vessels. At its boiling temperature the colour is also permanent.

* Haller Elem. Physiolog. vol. i. p. 51.

Infusion of galls produces no change in this muriatic solution, nor is its colour affected by carbonated alkalies, even when added in considerable excess.

It is rendered brown red by supersaturation with caustic potash, but not with soda, nor ammonia: these, and especially the latter, rather heighten its colour.

When considerably diluted with water its original colour is much impaired, and the green hue, which it always exhibits by transmitted light, becomes more evident.

In preparing this solution, I frequently employed the coagulum of blood cut into pieces, and digested in equal parts of muriatic acid and water, at a temperature between 150° and 200°. In three or four hours the acid was poured off, and filtrated. The clear solution was in all respects similar to that above described, although before filtration it appears of a dirty brown colour.

I evaporated a portion of this muriatic solution in a water-bath, to dryness; it retained its colour to the last, and left a transparent pellicle upon the evaporating bason, of a dirty red colour: this when re-dissolved in muriatic acid acquired its former tint, but the colour of its aqueous solution was nearer brown than red.

B. Sulphuric acid, diluted with eight or ten parts of water, forms an excellent solvent of the colouring principle of the blood.

It may be employed in a more concentrated state; but the bright colour of the solution is in that case apt to be impaired, and when more largely diluted with water, its action is slow and uncertain. Either the sediment of the colouring matter from the serum, or the crassamentum of the blood, may be indifferently employed in forming these solutions.

When dilute sulphuric acid is added to the colouring matter, it renders it slightly purple; and if no heat be applied, the acid when poured off and filtered, is colourless; so that dilute sulphuric acid when cold, does not dissolve this colouring principle.

One part of the crassamentum of blood cut into pieces, was put into a matrass placed in a sand heat, with about three parts of dilute sulphuric acid. It was kept for twelve hours in a tem-

perature never exceeding 212°, nor below 100°. After twenty-four hours the acid was filtered off, and it exhibited a beautiful bright lilac colour, not very intense, and teinted with green when viewed by transmitted light.

This solution is nearly as permanent as that in the muriatic acid. Some of it which has been kept for a month in an open vessel, often exposed to the direct rays of the sun, is very little altered.

When diluted with two or three times its bulk of water, the lilac tint disappears, and the mixture is only slightly green.

When exposed to heat, the colour gradually changes as the acid becomes more concentrated by evaporation, and when reduced to about half its bulk the lilac hue is destroyed.

The solutions of pure and carbonated alkalies when added in excess, convert the colour of this sulphuric solution to brownish red; but in smaller quantities they merely impair it by dilution.

C. Nitric acid, even much diluted, is inimical to the colouring matter of the blood.

A few drops added to the muriatic or sulphuric solutions gradually convert their colour to a bright brown, and larger quantities produce the same change immediately.

The action which this acid exerts upon the colouring matter under other circumstances is nearly similar, and always attended with its decomposition, so that my attempts to procure a red solution in this menstruum uniformly failed of success.

D. Acetic acid dissolves a considerable quantity of the colouring matter of the blood; the solution is of a deep cherry red colour. When somewhat diluted, or when observed in tubes of about a quarter of an inch bore, this solution appears perfectly green by transmitted light. In its other habitudes it nearly resembles the muriatic solution.

E. The solution of the colouring matter in oxalic acid is of a brighter red than those hitherto noticed; that in citric acid is very similar to the acetic solution, and with tartaric acid the compound somewhat inclines to scarlet. All these solutions exhibit the green hue, to which I have so often alluded, in a remarkable degree.

6. *Effects of Alkalies on the colouring Principle of the Blood.*

The caustic and the carbonated alkalies form deep red solutions of this substance, which are extremely permanent.

1. Solutions of pure potash, and of the subcarbonate, take up a large proportion of the colouring matter of the blood. The intensity of the colour of this solution, when concentrated, is such, that it appears opake, unless viewed in small masses, or in a diluted state, when it is of a bright red colour.

2. In soda and its subcarbonate, the solution has more of a crimson hue, which colour is extremely bright in its concentrated state.

3. The solution in liquid ammonia approaches nearer to scarlet than that in which the fixed alkalies are employed.

4. When these alkaline solutions are supersaturated with muriatic acid, or with dilute sulphuric acid, they acquire a colour nearly similar to the original solutions in those acids which have been above described.

5. Nitric acid added in small quantities, or even to saturation of the alkaline menstruum, heightens the colour of the three compounds; but when there is a slight excess, a tint of orange is produced, which soon passes into bright yellow.

6. The alkaline solutions may be evaporated nearly to dryness without losing their red colours; during the evaporation of the ammoniacal solution, the alkali flies off, and a brown-red solution of the colouring matter in water remains.

Having ascertained the above facts respecting the colouring principle of the blood, I next proceeded to examine how far it was susceptible of entering into those combinations which are peculiar to other varieties of colouring matter.

These experiments I shall detail in the order in which they were made.

1. Some pure alumine was added to a concentrated aqueous solution of the colouring matter of the blood, and after twenty-four hours the mixture, which had been frequently agitated during that period, was poured upon a filter, and the residuum washed with hot distilled water.

The filtrated liquor had lost much of its original colour; the

alumine had acquired a red tinge; it was dried at a temperature between 70° and 80°, during which it became brown.

2. Two hundred grains of alum were dissolved in four fluid ounces of a solution of the colouring matter, similar to that employed in the last experiment. The colour of the compound was bright red. Liquid ammonia was added, and the precipitate collected, and carefully dried. It was of a dirty red, and after some days exposure to light became nearly brown.

From these, and other experiments which I have not thought it necessary to detail, it appears that alumine will not form a permanent red compound with the colouring principle of the blood; I was therefore next induced to employ oxide of tin.

3. Fifty grains of crystallized muriate of tin (prepared by boiling tin filings in muriatic acid, and evaporating the solution,) were dissolved in four ounces of the solution of colouring matter, which immediately assumed a purple tint, and became afterwards brown. It was diluted with twice its bulk of water, and put aside in a stopped phial. On examining it three days afterwards, a small quantity of a bright red powder was observed at the bottom of the phial, which proved to consist of the colouring principle combined with the metallic oxide. A portion of this compound which has been kept in water for some weeks has undergone no change of colour; but when dried by exposure to air, it loses its brilliant tint, and becomes of a dull red hue.

To a compound solution of muriate of tin and colouring matter, similar to that employed in the last experiment, I added a sufficient quantity of solution of potash to decompose the salt of tin. The precipitate thus obtained was collected and dried by exposure to the air of a warm room. It was of a dull red colour, and has undergone no apparent change by exposure to the joint action of light and air for three weeks.

4. Finding that supertartrate of potash exalted the colour of the blood, I endeavoured to form a compound of it with that substance and oxide of tin, and thus, in some measure, to imitate the process in which cochineal is employed for the production of scarlet dye; but although a bright red compound is produced, when it is dried at a very moderate temperature its

colour becomes similar to that of the other combinations which I have described.

These experiments I repeated in various ways, occasionally applying the salt of tin as a mordant to woollen cloth, linen, &c.; but the brilliancy of the colour was never permanent.

5. Having observed that infusion of galls and decoction of oak bark do not impair the colour of the blood, I conceived that solution of tannin might answer the purpose of a mordant, as it is effectually employed by dyers in giving permanence to some of their red colours.

I accordingly impregnated a piece of calico with decoction of oak bark, and afterwards passed it through an aqueous solution of the colouring matter of blood. When dry, it was of a dirty red colour, nearly similar to that which would have been obtained had no mordant been applied: when however an alkaline solution of the colouring matter was employed, the colour was equal to that of a common madder red, and as far as I have been able to ascertain, it is permanent.

6. A solution of superacetite of lead was impregnated with the colouring matter of the blood. The compound was bright red: no spontaneous change took place in it, and on the addition of an alkali a white precipitate was formed, the fluid retaining its former tint.

From this and other experiments, in which it was attempted to combine oxide of lead with the colouring matter of the blood, it would appear that there is no attraction between those two substances.

7. The most effectual mordants, which I have discovered for this colouring matter, are some of the solutions of mercury, especially the nitrate, and corrosive sublimate.

Ten grains of nitrate of mercury (prepared with heat and containing the red oxide) were dissolved in two fluid ounces of a solution of the colouring matter of the blood. After some hours a deep red compound was deposited, consisting chiefly of the metallic oxide combined with the colouring matter, and a small portion of coagulated albumen. The remaining fluid had nearly lost its red colour.

The nitrate of mercury containing the black oxide produces

nearly similar effects, excepting that the colour of the compound is of a lighter red.

When corrosive sublimate is added to the solution of the colouring matter, its tint is instantaneously brightened, and it becomes slightly turbid from the deposition of albumen. If this be immediately separated by a filter, the liquor which passes through gradually deposits a deep red or purplish insoluble precipitate; and if it now be again filtrated the liquid is colourless, the whole of the colouring principle being retained in the compound which remains upon the filter.

By impregnating some pieces of woollen cloth with solution of nitrate of mercury, or of corrosive sublimate, and afterwards steeping them in an aqueous solution of the colouring matter of the blood, I succeeded in giving them a permanent red tinge, unalterable by washing with soap; and by employing the ammoniacal solution of the colouring matter, calico and linen may be dyed with the same mordant.

In these experiments I was much satisfied by the complete separation of the colouring matter from its solutions, which after the process were perfectly colourless.

Section VII.

Some Remarks on the preceding experimental Details.

From the experiments related in the second section of this paper, it appears that sulphuric acid effects changes upon the coagulum of chyle, similar to those which Mr. Hatchett has observed to result from the action of dilute nitric acid upon the coagulated white of egg. This last substance, however, is not convertible into gelatine by means of sulphuric acid, whereas in these respects the curd of milk resembles that of chyle: this circumstance, as well as the more ready solubility of the coagulum of chyle in dilute than in concentrated acids, points out a strong analogy between those two bodies.

The sweet taste of chyle naturally suggested the idea of its containing sugar*; but I am not aware of any direct experiments which have demonstrated its existence, and have therefore detailed minutely such researches as I have been enabled

* Fordyce on Digestion, 2d edition, p. 121.

to make upon the subject, hoping at some future period to render them more complete.

The experiments to prove the non-existence of gelatine in the serum of blood, will, I trust, be deemed sufficiently decisive: they show that that abundant proximate principle of animals is not merely separated from the blood, in which it has been supposed to exist ready formed, but that it is an actual product of secretion.

The proportion of iron afforded by the incineration of several varieties of animal coal, is much less considerable than we have been led to expect; and the experiments noticed in the fifth section show that it is not more abundant in the colouring matter of the blood than in the other substances which were submitted to examination; and that traces of it may be discovered in the chyle which is white, in the serum, and in the washed crassamentum or pure fibrina.

The inferences to which I have alluded, in the first section of this paper, are strongly sanctioned by these facts, and coincide with the opinion which has been laid before the Royal Society, by Dr. Wells*, respecting the *peculiar nature* of the colouring principle of the blood, and support the arguments which are there adduced.

That the colouring matter of the blood is perfectly independent of iron, is, I conceive, sufficiently evident from its general chemical habitudes, and it appears probable that it may prove more useful in the art of dyeing than has hitherto been imagined, since neither the alkalies nor the acids (with the exception of the nitric) have much tendency to alter its hue. The readiness, too, with which its stains are removed from substances to which no mordant has been applied, seem to render it peculiarly fit for the purposes of the calico-printer. I have not extended these experiments, nor have I had them repeated on a sufficient scale to enable me to draw more general conclusions respecting the possibility of applying them with advantage in the arts: this would have led me into too wide a field, and one not immediately connected with the objects of this society: the subject, however, appears important.

* Phil. Trans. 1797.

It is not a little remarkable that blood is used by the Armenian dyers, together with madder, in the preparations of their finest and most durable reds*, and that it has even been found a necessary addition to insure the permanency of the colour†. This fact alone may be regarded as demonstrating the non-existence of iron as the colouring principle of the blood, for the compounds of that metal convert the red madder to gray and black.

Whilst engaged in examining the colouring matter of the blood, I received from Mr. William Money, house surgeon to the general hospital at Northampton, some menstruous discharge, collected from a woman with prolapsus uteri, and consequently perfectly free from admixture of other secretions. It had the properties of a very concentrated solution of the colouring matter of the blood in a diluted serum, and afforded an excellent opportunity of corroborating the facts respecting this principle, which have been detailed in the preceding pages. Although I could detect no traces of iron, by the usual modes of analysis, minute portions of that metal may and probably do exist in it, as well as in the other animal fluids which I have examined; but the abundance of colouring matter in this secretion should have afforded a proportional quantity of iron, did any connection exist between them. It has been observed that the artificial solutions of the colouring matter of the blood invariably exhibit a green tint when viewed by transmitted light: this peculiarity is remarkably distinct in the menstruous discharge‡.

I hope that some of the facts furnished by the above experiments may prove useful to the physiological enquirer: they account for the rapid reproduction of perfect blood after very copious bleedings, which is quite inexplicable upon that hypothesis which regards iron as the colouring matter, and may perhaps lead to the solution of some hitherto unexplained phænomena connected with the function of respiration. There can, I think, be little doubt that the formation of the colouring mat-

* Tooke's Russian Empire, vol. iii. p. 497.

† Aikin's Dictionary, art. *Dyeing*, and Philosoph. Magazine, vol. xviii.

‡ I could discover no globules in this fluid; and although a very slight degree of putrefaction had commenced in it, yet the globules observed in the blood would not have been destroyed by so trifling a change.

ter of the blood is connected with the removal of a portion of carbon and hydrogen from that fluid, and that its various tints are dependent upon such modifications of animal matter, and not, as some have assumed, upon the different states of oxidizement of the iron which it has been supposed to contain.

On the Mercurial Plan of Treatment in Dysentery; with Observations on the same Practice as applied to Yellow Fever, and to the Remitting Fevers which often occur in Europe, as well as in the East and West Indies. By WILLIAM FERGUSSON, Esq. Inspector-General of Hospitals to the Army in Portugal. *Communicated by* Dr. MARCET, For. Sec. &c.

[From the Medico-Chirurgical Transactions, Vol. II.]

THE following paper was originally communicated to the Society in the form of a private letter, without any authority to publish it; but having been sent back to the author in Portugal, with a request that he would allow it to be published in the Society's Transactions, with whatever corrections or additions his subsequent experience might suggest, the original document was speedily returned by Mr. Fergusson, accompanied by a letter, of which the following is an extract:—

Lisbon, May 20th, 1811.

"I now return the paper, with full permission to make "whatever use of it the Society may think proper. I do not "give this permission lightly, for I have revolved and reconsi- "dered its contents with all my strength; and really find, that "every opinion it contains, so far from being changed, has "been gaining additional confirmation ever since I wrote it. "In a postscript will be found an extract of a letter from Dr. "Gray, Physician to the forces; who has been travelling over "the same field of military practice as myself, for an equal "number of years (now eighteen), only he has had the advan- "tage, which I never enjoyed, of spending a considerable time "in the East Indies. He decidedly concurs in the propriety of "mercurial remedies for dysentery; and in a late publication "by Dr. Gourlay, on the Climate and Diseases of Madeira, there "is a decisive testimony in favour of the mercurial practice."

Portsmouth, November 12, 1809.

* * * * * * * * * * * * *

* * It is only within a short time, and during the late campaigns in Portugal and Spain, that I have had recourse so frequently to Mercury. I confess myself to be partial to its use, though I may indeed have pushed it beyond the mark. The universal epidemic that there prevailed among every description of military in the field, was dysentery. This, when mild, admitted of an easy cure, by acting on the bowels with mild purgatives, and keeping up their action steadily, but not violently, for a few days. It was cured with nearly the same facility by acting upon the skin without purgatives, through the influence of active diaphoretics. In this way every regimental surgeon, looking to the number of recoveries from his sick list, believed that he possessed a cure for the disease, whether he followed the one or the other mode of treatment. The attack, however, sometimes began with such urgent and violent symptoms, that a power beyond either of these two, became necessary to save the patient; irreparable mischief otherwise ensued, from the violent inflammation, followed by ulceration and thickening of the colon and larger intestines; and the patient, if he survived the acute attack, sunk afterwards a miserable victim of chronic disease. It is probably this form alone, of which I sustained two very severe attacks, that deserves the name of true acute dysentery; the others were local diseases of the lower intestines, in which the general system was little affected, or did not at all sympathise. In the aggravated form, there appeared one never failing symptom which always served me as a guide and diagnostic. The urine was high coloured, even green, scanty, and pungent; and though there were no other discoverable signs of hepatic affection, this was my signal for beginning and pushing the use of mercurial remedies. Half a grain of calomel, with one grain of ipecacuan, was given every hour. This never aggravated the abdominal pains; on the contrary, appeared to alleviate them; or if it did not, some mild saline purgative, as castor oil largely diluted with mucilage of gum arabic gave ease, and permitted the continuance

of the mercury till the gums were affected. This generally took place in forty-eight hours, when a solution of the disease might be looked for with confidence; and of which one of the most certain precursors was the urine reassuming its natural condition. In a few cases, and really in a very few, the disease did not yield after the mouth became sore; and these were then found to be of an obstinate and incurable nature. I had few opportunities of seeing many of that description, but those that came under my view were elderly soldiers, who, from former abuse of spirituous liquors, might be supposed to labour under previously obstructed viscera. In others, again, the use of mercury was neglected, and the patient died. The dissection then exhibited a miserable mass of disease in the great intestines. The colon, its descending portion particularly, being thickened, knotted and ulcerated to an inconceivable degree. The smaller intestines showed little or nothing of these appearances, and we often lost even the traces of disease, till we came to the liver, which uniformly was blackish, hard and wasted; the gall bladder flaccid, and about half full of thin watery bile.

I had an early and painful lesson in the mercurial practice; for in preparing to land in Portugal with Sir John Moore's army, and being exposed to the night dews on the water after a dreadfully hot and fatiguing day of preparation, I was attacked suddenly and violently with dysentery. Purgatives gave only a momentary relief, and notwithstanding the fair trial of other approved remedies, the complaint continued unabated, and in three days I was reduced to extremity. A friend enjoined the immediate use of calomel in grain doses. I had before taken one or two large doses as purgatives, which probably quickened the effect; for in about fifteen hours, I perceived the coppery taste in my mouth, and from that moment I may say, I felt that the disease was gone: my spirits and appetite revived, and I no longer thought of dying.

I need not say, that this early lesson was an instructive one. The whole army was more or less dysenterically affected, but by administering mild diluted purgatives to the slighter cases, and boldly mercuralising, even by inunction, which I believe to be the best practice, the severest, the deaths from the disease, in the regimental hospitals, where the greater part of the

sick were treated, scarcely exceed one in two hundred. Many of these were of course trifling, but all were dysenteries; and showed the diagnostic symptoms of tenesmus and of mucous evacuations without any admixture of bile or fæces. In some the disease might be said to be entirely local, and confined to the lower part of the intestinal canal. The patient felt no constitutional disturbance, and could do his duty; but was troubled with an irritation, like that from ascarides, about the rectum; to which, if he yielded, the irritation was increased, with frequent, scanty, mucous stools, and tenesmus. My direction was always, if possible, to resist the inclination to enforce the passage of the natural fæces, at least once a day, by some purgative, and to wear a broad flannel roller swathed round the whole of the abdomen: this often succeeded without confinement.

It may strike you as strange, that in almost every stage of the acute disease, I considered opium to be hurtful, and even dangerous. The temporary ease which it afforded from the tormina of the bowels, was generally succeeded by worse symptoms; and besides, the same degree of relief was much more effectually obtained by mild purgatives, which were otherwise productive of permanent benefit. Astringents of every kind, during the acute stage, were even worse than opium. When the excessively acrid discharge that came from the bowels was examined, it was easily to be seen, that it could not be locked up in the intestines with impunity, and that the patient could only expect to be easy from having its exit facilitated. During convalescence, and when the natural passage of the fæces was restored, there could be no objection to the use of any of the tonics and astringents; but amongst these opium held no place.

From all these facts, I came to an early conclusion, that though it was little discoverable from any symptom but the condition of the urine, yet that we were to look to the liver, principally, for the source of the disease. The apparent seat of the complaint was to be seen in the intestines themselves; and the action of purgatives upon them may be supposed sufficient to change the diseased, and to induce a new action; but still the grand organ of the bile was to be specifically stimulated,

and its ducts steadily emulged, and duly opened by the operation of mercury, before a cure could be expected.

Dysentery, from my experience of it in Holland, the West Indies, and South of Europe, I believe I can declare to be in no case contagious. I never saw any thing like it, except in Holland, after the weather became cold, when it was seen as a symptom or irregular local form of typhous fever; but more could not then be said, than that it was about to be swallowed up in the prevailing epidemic, and formed a combination, rather than a distinct type of disease.

True dysentery is the offspring of heat and moisture; of moist cold in any shape after excessive heat: nothing that a man can possibly eat or drink would ever give him true dysentery. A chill from dry cold air would produce a different affection; but the more penetrating forms of moisture, such as lying on damp ground in the hot season, or being exposed to the night dews, or to a stream of chilling exhalations after violent rains, when the system has been relaxed by previous heat, seldom fail to bring it on. Marsh miasma would beget fevers of its own generic type, and moist cold alone, not preceded by heat, would manifect its action by inducing rheumatic, pneumonic, or catarrhal fever, according to circumstances. I know not, whether the ascertained indispensability of previous heat in the production of dysentery, will be admitted as a proof of the hepatic system always participating in, and being, in fact, a seat of the primary symptoms. Certain it is, however, from the state of the urine, that there must at least be spasm, or collapse and inaction, of the biliary ducts; and the wasted appearance of the liver, far beyond what could be expected from general constitutional exhaustion, gives countenance to the supposition of its functions having been disordered and impeded.

There is another disease, happily for this country little known amongst us, where mercury, not empirically used, but duly applied at the proper stage and period, I hold to be a great, and probably the *only* remedy upon which much reliance could be placed—I mean the remittent fever, which rages throughout the world, wherever a marshy surface is exposed for a sufficient length of time to a powerful sun. It is by far the

most universal of all the forms of fever, and whether it comes out an ague from the fens of Lincolnshire, the endemic remittent (commonly called the yellow fever) of the West, or the Jungle or Puckah fever of the East Indies, it is only a modification of effect from the same cause. Its mode of invasion constitutes at once its decided difference from our typhous fever. In typhus, the symptoms insidiously creep upon the patient; but in remittent fever the attack is at once powerful and decided, and all the powers of the system appear to be called into action against the newly applied poison. We cannot know whether this increased action (commonly called re-action) becomes, from its violence, dangerous to life, or whether the unsubdued power of the original cause finishes the work of destruction; but we know that the whirl and hurry of the circulation, if not moderated by artificial means, soon terminates in deposition upon, or determination to, some of the important vital organs, to the large abdominal viscera of the liver and spleen, more particularly the first. In hot climates, this determination is generally fatal; but in the remittent fever of Europe, more particularly that of Holland, it is often the commencement of chronic, irregular fever, with imperfect convalescence, and frequent relapses; from which (the acute symptoms being past) at first little danger is apprehended. Our practice should be regulated accordingly. Mercury, during the violence of the first symptoms, would in all probability have done mischief, by increasing the effects of general stimulation; but in the second stage, it must, from its known qualities, have powerful effects in rendering pervious the obstructed viscera: without it, I believe that tonics and stimulants would only do harm, by rousing the system into undue efforts, from which it must sink exhausted; but duly combined, the vital powers may be made to go hand in hand with the artificial action of the mercury. The liver, being the first object of congestion and determination, it might appear good practice, while the general invasion is kept in check by the vigorous means of depletion and refrigeration from without, to use calomel for the purpose of keeping open and emulging its inner emunctories; but I conceive this method would be too tedious to serve any good purpose, and that the

treatment at the beginning ought to be purely and generally antiphlogistic.

A question may here arise, respecting the propriety and extent of venesection. When used early, and before determination has been marked, I hold it to be the most powerful means of preventing the bad consequences that may otherwise be expected to ensue. It ought, therefore, in the young, sanguineous soldier, to be generally and decidedly practised, on the first invasion. If it has no other good effect, it will, at least for a time, enable the practitioner to hold the reins, and render the system more amenable to the action of remedies, more particularly sudorifics; but if delayed till congestion has taken place, it will then enfeeble the vital powers, and diminish the means and chances of the obstruction being overcome.*

The application of cold under any chance or suspicion of obstructed viscera, may form another question, but it can never surely be unsafe to diminish accumulated heat by any means not too violent; and though I should not fear to use the cold effusion, I doubt not that simple ablution or spunging would be perfectly effectual.

This chronic and irregular form of the Walcheren fever, is now treated at many of our military hospitals with success by mercurials only; and as the medical officers become enlightened by inspecting the bodies of those that die, I doubt not that it will become universal. It has been long my belief, that remittent fever does not endure many days without producing the effects above mentioned. As in the yellow fever of the West Indies, the patient may die in this country from the violence of the general symptoms; but I believe that death is still more certainly and frequently induced, by determination to, and injury of, some vital organ. The liver, I am convinced, was there primarily and principally affected. The patient could not live till it suppurated and ruptured, but it was always enormously

* The superstitious disuse of the lancet at the beginning of soldiers' diseases, which are then almost always inflammatory, amounting almost to a proscription of the remedy, through the prejudice of the schools, and the false inferences deduced from its effects on the debilitated subjects of private life, has caused more deaths, or at least prevented more lives from being saved, than any other fault in military practice.

congested, and its secretions so changed that, in place of the natural balsamic bile, it appeared to distil poison into the intestines. When we opened the bodies, the gall bladder was always found impacted with a black, tenacious, pitchy substance, as thick as common tar, and, at its communications with the intestines, they (the intestines) were in a state of low, erysipelatous, even semi-gangrenous inflammation. If we could suppose the albuginea of the eye in a jaundiced person to be in a state of inflammation, it would give the exact idea of their appearance. The patient seemed to die through the effect of incessant vomiting; the stomach was also found in a state of inflammation; but as this symptom seldom shewed itself till the disease was considerably advanced, we may suppose that the stomach being sympathetically affected by the presence of this acrid bile in the neighbouring intestines, regurgitation was produced by nausea; and that thus the poison was first received into that viscus whence it could never be expelled, as every effort for that purpose produced an augmentation of the original quantity. Were the heat as great in Walcheren, we should no doubt find the liver as much a sufferer, and an *agent*, as at St. Domingo; for in Portugal, after the battle of Vimeira, I met with the purest cases of yellow fever, in some regiments encamped amongst marshes; and at Sheerness in Kent, during the autumnal heats of the year preceding, while I was the Inspector of the districts, fevers occurred, which, for violence of symptoms, and strongly marked hepatic affection, might justly have been classed with West India diseases.* The defective sensi-

* The endemic remittent, though the most general, is not the only form of yellow fever; and the most dreadful mortality has ensued from that disease, where it was impossible even to suspect the agency of miasmata in any shape. In fact, all the young, the sanguineous, and robust, were its victims, and could not be saved from its attack, as long as they continued to possess European vigour, and until they had been relaxed by the climate, and entirely lost the rigidity of muscular fibre which they had brought from their own country. The invasion, progress, and termination of this type of fever, seemed to follow the same course as those of the endemic remittent, although it must have arisen from a different source, because it broke out where miasmata could not exist, and where exposure to the solar rays was most evidently the exciting cause. Amongst our unfortunate soldiers, intemperance, costiveness, plethora, youth, and the highest tone of muscular fibre, under

bility of the liver, more particularly under disease, has for ages deluded the dying patient and his medical directors. It was the current belief, till lately that their eyes have been opened by freer communication with the East, that liver complaint never occurred in the *West* Indies; and while I was there, it was common for the practitioners to boast of their curing agues and dysenteries by mercury, without their having the smallest suspicion of the true source of the disease.

In the great field of military practice, I am not disposed to abandon my opinions in favour of mercury, notwithstanding that in private practice it may often be very empirically and absurdly resorted to. I have seen the same amongst ourselves in the West Indies, where some of the advocates for mercury, as if to save themselves the trouble of thinking, blindly and preposterously pushed its use under all circumstances.

I may conclude the recapitulation of my creed by saying, that in dysentery I would use it to rouse from inanition, or inaction, the torpid liver, and restore to the smaller intestines their healthful bile; for without it they perform no duty, but

every supposable excitement from the senses, all predisposed to disease, and prepared the human frame, as if it had been a combustible body, to explode into fever before the torch of the sun. Why the solar rays do not exert a corresponding baleful influence on the sands of Egypt, or in other climates equally hot or hotter than the West Indies, or why the same effects were not felt even there to the same degree previous to the year 1793, is beyond our ken; and in all probability will never be discovered to us, any more than those other conditions and modifications of the atmosphere, which, independent of the qualities of soil, afflict us with influenzas and other epidemic diseases. When I first went to the West Indies, I had no doubts on the subject, because the contagious nature of Yellow Fever formed as much an article of my creed, as that of plague or small pox does now; but I had been there only a very short time, when, in spite of my prejudices, I was obliged to open my eyes to the irresistible conviction that broke in upon me from all quarters, of its not being capable of being propagated by contagion; and during the whole time that I remained in the country (several years) I never saw the smallest reason to alter that opinion. I do not pretend to say, that the punishment which is ordained against the undue accumulation of human effluvia, and neglect of ventilation in crowded ships, hospitals, or prisons, would not be manifested by the production of temporary pestilence in the West Indies, the same as in any other climate; but I fully believe, that the Yellow Fever, under ordinary circumstances of ventilation and cleanliness, is never contagious.

permit the stricture on the larger ones to continue unresolved, and thereby run its course of obstruction and ulceration: and in remittent fever, when the violence of the acute stage has been past, I should equally depend on its specific stimulating power, to render pervious the obstructed viscera. Relapses would not then occur; for the axe will have been laid to the root of the evil; and in dysenteries cured by mercury, the same happy result may be expected to attend. Experience at least tells me so, as I have seen many a patient restored to vigorous health, with regular bowels, within a few weeks after having sustained an attack that might have been supposed to render him valetudinary for years. I have found few agents of much power in the materia medica; but mercury I hold to be one which, when skilfully wielded, may be applied to accomplish the most important purposes of medical practice; but exactly in the proportion that it is powerful, so is the danger of using it needlessly and improperly, or of committing its administration to unskilful hands; and I must own, that I have seen it commit sad havoc even in dysentery, when prescribed for slight cases, and persisted in till it became difficult to distinguish between the disease and the mercurial action in the bowels.

WM. FERGUSSON.

(Postscript.)

Lisbon, May 20, 1811.

When the foregoing letter was sent out to me here about a month ago, for correction, preparatory to its appearing before the public, I showed the contents to my friend Dr. Gray, who has favoured me with the following observations upon it, dated April 14, 1811.

" I have read your paper on dysentery with much interest, from the striking coincidence in your practical views with what has fallen under my own observation, when with the army in the East Indies, as well as in different parts of Europe. Indeed I have also long been an advocate for the employment of mercury in dysentery, from ample proofs of its success both in the acute and chronic stages, only delaying its use in the former, till the milder modes of treatment are either found inadequate, or the disease assumes an alarming aspect.

Experience, however, has led me to trust chiefly to the introduction of mercury into the system, by means of friction, in the proportion of ʒi. of the strong mercurial ointment twice, thrice, or even four times in the day, according to the urgency of the case, and carried the length of not merely affecting the gums, but inducing ptyalism to the extent of an English pint, or even more, daily; which effect must be kept up for a week or longer, if the disease do not give way, and the patient's strength will bear it: and I can safely aver, that, in this way, mercury has not only very often succeeded after the failure of every other remedy and mode of treatment that could be devised, but has even rescued some desperate cases, when no reasonable hope of recovery could be entertained. When the mouth becomes what the patients term *very sore*, though without much spitting, there is pretty generally an abatement of the dysenteric symptoms; but the mercury requires to be pushed the length mentioned, effectually to stop the disease. Still it must not be imagined, that I have not found mercury to fail in dysentery; for unfortunately the reverse is the fact. But I will venture to assert, that the mercurial plan, employed before the organic mischief is irreparable, or the debility and emaciation irrecoverable, affords the most successful mode of treatment hitherto employed; not merely in the inter-tropical dysentery, or rather hepatirrhœa, (when the acrid biliary secretion, irritating, and eventually ulcerating, the intestines, is often the first symptom of a diseased liver) but also in dysentery, as described by nosologists, and occurring during European campaigns; the symptoms of which differ considerably from those of the former, and in which I cannot help thinking that the liver is seldom to be looked to for the source of the disease. When the mercurial plan has failed, change of climate, when practicable, and not delayed till the last extremity, sometimes succeeds in bringing about recovery.

"I have thus briefly stated the result of what I have seen and practised, in the treatment of a disease, that often proves the scourge of armies; without indulging any conjectures on the *modus operandi* of the remedy." (Signed)

J. GRAY.

"In regard to the treatment of this disease, (says Dr. Gourlay) I hold it of the utmost consequence that the proximate cause, the peculiarly morbid inflammation of the intestines, be constantly kept in view; for it appears from dissection, to be in every case directly or indirectly the cause of death. That plan of cure then ought to be the best, which forms its indications from the view of removing this proximate cause. According to this plan, to remove the peculiar morbid inflammation, to unload the bowels, to relieve uneasy symptoms, and to restore to the intestines their healthy action, are the general indications to be formed. The best and most effectual means of answering at once all these indications, according to my experience, is the use of calomel alone. This medicine, exhibited in doses of six, eight, or ten grains, repeated at intervals, determined in their length of time by the previous effects of its action, and the other circumstances of the patient, has been uniformly found to unload the bowels, and to keep them open; to relieve the uneasy symptoms of tormina and tenesmus, and even of vomiting, if present; to raise the pulse, and remove that languid look and general depression, so constantly attendant on this disease. To judge from these effects, the probability is, that it also operates in some peculiar way; in a way, perhaps, analogous to its mode of action, when applied externally to an ill-conditioned ulcer, upon the morbid inflammation of the intestines, so as to remove it. In regard to astringents and opiates, I have seldom found occasion for such articles, unless where the free evacuation of the intestines was neglected in the first instance, or large doses of such debilitating cathartics as aloes, colocynth, jalap, rhubarb, or clysters, containing acrid materials, had been administered for this purpose. Indeed I have found calomel to be not only the best remedy in the beginning, for unloading the bowels, and relieving uneasy symptoms, but also in the latter stage, to be the best and softest anodyne." Observations on the Natural History, Climate, and Diseases of Madeira, during a period of eighteen years. By Wm. Gourlay, Fellow of the Royal College of Physicians, Edinburgh, and physician to the British factory at Madeira, London, 1811. 8vo. p. 135, 136.

Some account of the Effects of Arsenic in counteracting the Poison of Serpents. Communicated in a letter from Mr. J. P. IRELAND, Surgeon to the fourth battalion of the sixtieth regiment of foot, to THOMAS CHEVALIER, Esq.—Read before the Medico-Chirurgical Society, June 12, 1811.

[From the Medico-Chirurgical Transactions, Vol. II.]

SIR,

HAVING heard you mention in your lectures, "that the Indians were in the habit of administering arsenic in large doses, after the bites of venomous animals; and that you would strongly recommend to gentlemen who might have opportunities of trying its effects in such cases, to exhibit it, in order to ascertain its powers," I resolved to make trial of it whenever an opportunity offered; and I have great pleasure in having it in my power to acquaint you with the following important facts, which occurred under my own observation during several years residence in the West Indies.

In some of the islands at present in our possession, venomous serpents are very numerous, and one of the most deadly in its bite is found in the island of St. Lucia; it is from three to six feet in length, and appears to be the *Coluber Carinatus* of Linnæus. Its fangs are from one and an half to two inches long;* and the wound inflicted by them is generally of considerable extent.

On my arrival in the island, I was informed that an officer and several men, belonging to the 68th regiment, (then quartered there, and to which I was attached,) had died within a few months from the bites of those destructive animals; that every thing had been tried by the attending medical men, to no purpose, as all the patients had died, some in six, and others in about twelve hours, from the time of their receiving the wound.

A case, however, soon came under my own care, and as

* Several specimens of the fangs I brought home with me have been unfortunately mislaid. I hope, however, to be able to show them to you soon.

nothing that had been done before seemed to be of any avail, I determined on trying the effects of arsenic to its full extent.

CASE I.

Jacob Course, soldier in the York light infantry volunteers, was bitten in the left hand, and the middle finger was so much lacerated that I found it necessary to amputate it immediately, at the joint with the metacarpal bone.

I first saw him about ten minutes after he had received the wound, and found him in a torpid, senseless state: the hand, arm, and breast of the same side were much swelled, mottled, and of a dark purple, and livid colour. He was vomiting, and appeared as if much intoxicated. Pulse quick and hard: he felt little or no pain during the operation.

The wound being dressed, and the patient put to bed, I ordered a cathartic clyster, and the following medicine to be taken immediately: ℞ liq. arsenic. ʒii;* tinct. opii gt. x. aq. menth. pip. ℥iss.; which was added to half an ounce of lime juice, and as it produced a slight effervescence, it was given in that state: this remained on his stomach, and was repeated every half hour for four successive hours. In the mean time the parts were frequently fomented with common fomentation, and rubbed with a liniment composed of ol. terebinth. ℥ss; liq. ammon. ℥ss. and ol. oliv. ℥iss. The cathartic clyster was repeated twice when the patient began to be purged; the arsenical medicine was now discontinued. He became more sensible when touched, and from that time he gradually recovered his faculties; he took some nourishment and had several hours sleep.

The next day he appeared very weak and fatigued; the fomentation and liniment were repeated. The swelling diminished gradually; the natural colour and feeling returned, and by pro-

* The Solutio Mineralis Arsenici was carefully prepared by myself agreeably to Dr. Fowler's prescription; which directs sixty-four grains of arsenic, and as many of the fixed vegetable alkali, to be dissolved in a sand heat, and the solution to be made an exact pint, so that two drachms contain one grain of arsenic in solution.

per dressings to the wound, and attention to the state of his bowels, he soon recovered and returned to his duty.

CASE II.

—— Dover, a black soldier in the 3d West India regiment, was bitten in the left hand; the swelling was not so extensive as in the former case, and the discoloration was not so strongly marked. I saw him within a few minutes after he was bitten, and vomiting, senselessness, and torpor, had commenced. The wound inflicted was not of so large extent; I removed the torn edges of the lacerated integuments, dressed the wound, and gave him the arsenical medicine precisely as in the former case; the fomentation and liniment were also applied; the cathartic clyster given every hour, and the medicine repeated every half hour for four hours, when purging came on, and the medicine was discontinued, after which he had some hours repose.

The next day he appeared less debilitated, and he soon recovered, and returned to his duty.

CASE III.

Thomas Rally, of the 68th regiment, was bitten in the calf of the right leg, and brought to the hospital in nearly the same state as Jacob Course; the ragged edges of the integuments were immediately removed, the wound dressed, and the arsenical medicine administered; a cathartic clyster was ordered, and the fomentation and liniment applied. After he had taken the medicine every half hour for three hours, severe vomiting came on, so that the stomach rejected every thing that was taken: the cathartic clyster was, however, repeated every hour for four hours, when purging came on. In about two hours afterwards the vomiting ceased, and he took tinct. opii gt. xx.; spt. æther. vitr. gr. xv.; aq. menth. pip. ʒiss.; after which he became quiet, and remained so for several hours.

Next day he was still much debilitated, and had much difficulty in voiding his urine; it was, however, drawn off with a catheter twice a day for two days, and fomentations applied to the region of the bladder. On the third day every aggravating

symptom began to abate, and from that time he gradually recovered and returned to his duty.

CASE IV.

Patrick Murphy, of the 68th regiment, was bitten in the wrist. I saw him within a few minutes after the wound had been received. The hand and arm of the same side had begun to swell, and were even mottled; but vomiting had not come on. I removed the torn edges of the integuments, dressed the wound, and gave him the arsenical medicine. The cathartic clyster was also ordered, and the fomentation and liniment applied; he took the medicine every half hour for three hours, when he appeared much recovered, and it was discontinued. The symptoms did not run so high as in any of the former cases. The fomentation and liniment were continued for two days, from which period he gradually recovered and returned to his duty.

These were the only cases I had an opportunity of seeing during my stay in St. Lucia; but some time after I went to the island of Martinique, where a venomous serpent is found of a smaller size, from one to two, and two and a half feet long, and its fangs about an inch in length, the bite of which is as deadly as of those found in St. Lucia. I was present when a soldier belonging to the 63d regiment received a wound in the leg from one of those serpents, and I requested the surgeon of the regiment to allow me to try the effects of arsenic; he was very glad to give me the case, as he had not before seen one of the kind.

The patient was treated precisely in the same manner as those in St. Lucia, and when I left the island on other duty, a few days afterwards, I had the pleasure to leave him so well, that I do not entertain a doubt of his having perfectly recovered.

From this it appears that very happy effects may be produced by the administration of arsenic in large doses, in cases of this nature, as every one in which I tried it recovered; and, I trust, these facts will be the means of throwing some light on a subject of the greatest importance to the inhabitants of our

colonies, and will be found to merit the attention of those who are more competent to reason on the nature of the effects thus produced.

I have the honour to be, sir,
Your most obedient and humble servant,
J. M. IRELAND,
Surgeon 4th batt. 60th regt.

London, October 21, 1811.

NOTE BY MR. CHEVALIER.

I was induced to recommend the trial of arsenic in these cases from the facts recorded in Dr. Russell's History of Indian Serpents, on the authorities of Mr. Duffin and Mr. Ramsay, and of which a good account may be found in the London Medical Review and Magazine for March and April, 1799. From these it appears that the Tanjore pill, of which arsenic is in all probability the chief ingredient, is exhibited with considerable success in India after the bites of venomous serpents. The composition of this medicine is as follows: "White arsenic, roots of vellinavi, roots of neri-visham, kernels of nervalam, pepper, quicksilver, of each an equal quantity. The quicksilver is to be rubbed with the juice of the wild cotton till the globules become invisible. The arsenic, being first levigated, and the other ingredients reduced to a powder, are then to be added, and the whole beaten up together with the juice of the wild cotton to a consistence fit to be divided into pills of six grains each." Each pill, therefore, contains nearly one grain of arsenic, which is given in the state of white oxyd. It was a matter of doubt with me, before I received Mr. Ireland's communication, whether the other ingredients might not have some share in moderating or counteracting the immediate effect of arsenic on the stomach itself. But it did not appear at all unreasonable to suppose that where such sudden and extreme debility is produced as takes place from the bites of serpents, a much larger dose of a a powerful remedy might be taken with impunity than could be borne in any ordinary state of the human frame. In Mr. Ireland's cases, the arsenic was also given in the state of white oxyd, as the liquor arsenicalis would be decomposed by the lime juice; but the medicine being swallowed instantly as that decomposition was effected, the arsenic would be more diffused in the stomach than if exhibited in a pill; and perhaps this mode of giving it is on that account to be preferred.

In two out of three of the successful instances related by Dr. Russell, the Tanjore pill produced copious vomiting; in one purging also, and in one the operation was gentle by stool and perspiration only. In those now read to the Society it seems to have operated chiefly by purging, which was very judiciously promoted by repeated cathartic clysters.

It remains for future observation to determine whether any, and what advantage would arise from giving the arsenic in solution instead of in the state

of oxyd; and whether this powerful remedy might not be employed with success in tetanus and hydrophobia. Dr. Russell mentions his having given the Tanjore pill to fourteen persons bitten by mad dogs. It operated by purging; but as the symptoms of hydrophobia do not appear to have supervened, the efficacy of the medicine in that dreadful disease still remains for future experiments to determine.

T. CHEVALIER.

Note referring to Mr. CHEVALIER'S *Case of Lithotomy, p.* 265.

Dr. WOLLASTON has been so good as to analyse the matter voided by the patient, whose case is described in the above paper, (see p. 270), and finds it "to be principally a remnant of a coagulum of blood, surrounded, as might be expected, by a medley of those concretions which usually form on any substance retained in the bladder; viz. triple phosphate of magnesia, phosphate of lime, and uric acid."

T. CHEVALIER.

A chemical account of various Dropsical Fluids; with remarks on the nature of the Alkaline Matter contained in these Fluids, and on the Serum of the Blood. By ALEXANDER MARCET, M.D. F.R.S. one of the physicians to Guy's hospital.

[From the Medico-Chirurgical Transactions, Vol. II.]

The limits of the present number would not permit us to introduce Dr. Marcet's very ingenious experiments; but we avail ourselves of the general view of their results, as drawn up by that able chemist and physician; believing that such will be acceptable to those who take an interest in physiological inquiries.

As there are but few readers to whom chemical details are interesting, and as most of them may be glad to obtain some general notions of the results, without wading through a mass of experimental evidence, it may be proper to retrace, in a few words, the leading points of this inquiry.

It appears, in the first place, that the prevailing animal substance, not only in serum, but in all the morbid fluids which have been examined in this essay, is albumen, or coagulable matter; which substance, however, these fluids contain in very different proportions.

In all of them, also, another kind of animal substance, which may be called muco-extractive matter, (from its being incoagulable, and from its being soluble in water or other menstrua), is uniformly discoverable.

Gelatine, it would appear, is not discoverable in any of these fluids; a singular circumstance, from which it seems natural to infer that the formation of gelatine is the result of a specific secretion.

In some of these fluids, namely, those of ascites, hydrothorax, hydrops pericardii, hydrocele, and that which is sometimes effused in the thyroid gland, the albuminous matter is so considerable as to render them coagulable; that is, convertible into an uniform semi-solid mass, by the agency of acids, or by a temperature of 165 degrees. In others, on the contrary, namely, in the fluid of spina bifida, of hydrocephalus, and of hydatids, the quantity of albuminous matter is so small, as scarcely to be rendered visible by heat or acids.

The specific gravity of these fluids is likewise remarkably various. That of the fluid of hydrocephalus, for instance, is under 1007; whilst that of serum sometimes exceeds 1032. Different specimens of the same fluids are also found to vary in their specific gravities. This is especially conspicuous with regard to the serum of blood, some specimens of which do not exceed 1024, whilst others rise to 1032,5. Even in the same individual, and during the continuance of the same disorder, differences of specific gravity are observable in serum obtained at different periods. Thus, also, it is found that dropsical fluids from different cavities of the body, collected after death from the same individual, differ materially in regard to their specific gravities.

Upon further examination it is found that these differences affect principally, and perhaps exclusively, the animal matter of these fluids; the saline matter contained in them not being subject to similar variations.

These saline contents, on the contrary, are found to be remarkably uniform in the different fluids, both as to their proportions and their chemical nature; and the same uniformity prevails whatever differences there may exist in these fluids in regard to the animal matter they contain. Thus, serum of

blood, and the fluid of hydrocephalus, yield the same saline substances, and very nearly in similar proportions, although the one contains about eighty times as much animal matter as the other.* The proportion of saline matter yielded by the various animal fluids concerned in this inquiry, may be generally stated to be between 8 and 9 grs. in 1000 grs. of fluid.

The particular saline ingredients contained in all these fluids appear to be, muriat of soda, muriat of potash, sulphat of potash, soda, and phosphats of lime, iron and magnesia. And a mass of 100 grs. of these salts appears to consist of about 72 grs. of muriat of soda, mixed with a little muriat of potash; between 18 and 20 grs. of soda, brought to the state of subcarbonat; and a mixture of 8 or 10 grs. of sulphat of potash, phosphat of lime, phosphat of iron, and phosphat of magnesia. Potash, therefore, as Dr. Pearson first stated, is present in the animal fluids; but I believe I have satisfactorily shown, that it exists in the state of muriat, or sulphat; soda being the only alkali discoverable in an uncombined state.

The caustic alkali contained in these fluids appears to be combined with their animal matter, the properties of which it modifies in a manner which is not yet well understood:† and it would seem that the proportion of uncombined alkali to that of the other salts is greater, as the specific gravity of the fluid is more considerable.

These circumstances, and in particular the remarkable variations which are observed in the proportions of animal matter contained in the blood of different persons, or of the same person at different periods, derive considerable interest from their probable connexion with health and disease, and from the new pathological views which a full investigation of the subject might suggest.

* A table, exhibiting the general results of all the above analyses, will be found at the end of this paper.

† Mr. Brande, in his "Observations on Albumen, &c." published in the Philosph. Transact. for 1809, has made an ingenious attempt to explain the agency of alkali in albuminous fluids.

TABLE

Showing the proportions of saline and animal matter in various dropsical fluids, and in the serum of blood.

		In 1000 *grs. of Fluid.*		
	Specific Gravity.	Total of Solid Contents.	Quantity of Animal Matter.	Quantity of Saline Matter.
		Grains.	Grains.	Grains.
Fluid of Spina Bifida	1007	11,4	2,2	9,2
——— Hydrocephalus . . .	1006,7	9,2	1,12	8,08
——— Ascites	1015	33,5	25,1	8,4
——— Hydrothorax . . .	1012,1	26,6	18,8	7,8
——— Hydrops Pericardii .	1014,3	33	25,5	7,5
——— Hydrocele	1024,3	80	71,5	8,5
Serum of Blood*	1029,5	100	90,8	9,2

* The specific gravity here stated is an average. That of the particular specimen under examination was 1024,5.

SELECTED REVIEWS.

Cases of Apoplexy and Lethargy; with Observations upon the Comatose Diseases. By J. CHEYNE, M. D. Fellow of the Royal College of Physicians, Edinburgh, of the King and Queen's College of Physicians in Ireland, &c. &c. London 1812. 8vo. pp. 224.

[From the Edinburgh Medical and Surgical Journal, for July 1812.]

THIS is the work of a diligent and faithful observer, who has already distinguished himself as a clinical student. Early introduced to extensive and general practice, with a zeal and industry highly praise-worthy, Dr. C. has continued to note the cases of his patients, to record any important observation, to study, in detail, the living features of diseases in the sick-chamber, and to trace the connexion between symptoms and disorganization, by inspecting the bodies of the deceased. In thus contemplating the phænomena of hydrocephalus acutus, and preparing the materials of his essay on that disease, his attention was naturally directed to the general pathology of the brain; and the work now before us is, in part, the offspring of this study. That it had become necessary to reconsider the history and treatment of apoplexy, will very readily be acknowledged, when we recollect the late controversy which arose out of a dispute on a point of practice, between Dr. Langslow and Mr. Crowfoot, and with what animosity and contradiction of fact and argument, that controversy was maintained in the journals of the day. The argument too of Dr. Fothergill against blood-letting in apoplexy, had, we believe, influenced the understanding of too many practitioners, and restrained the use of the most powerful remedy we possess for the prevention or cure of this disease. To those who have entertained any doubts on this subject, we recommend a careful study of Dr. Cheyne's cases and observations. The work is truly clinical, and completely instructive on this point. The author is merely the historian of what he has seen, of what, indeed, all may see who choose to open their eyes. He has no particular conceit or hypothesis to deform the picture, and his obser-

vations are generally inductive, and when otherwise, are hazarded with the most becoming candour and modesty. The general history of apolexy is good, and conveys a very just picture of the disease. It contains, however, no observation of sufficient novelty or importance to detain us. We wish, perhaps, that Dr. Cheyne had dwelt more particularly on the presaging signs and symptoms of disease which mark the apoplectic diathesis, and precede often, for a long time, an impending attack.

His account of the morbid appearances exhibited on dissection claims more particularly the reader's attention.

In dividing the scalp, there is often a great flow of blood from the occipital and frontal veins; indeed, during the whole of a dissection, the blood flows from all parts of the head; first from the superficial veins, and then in streams from the sinuses, so that the quantity collected amounts to one or two pounds, or even more. The dura mater is sometimes thickened, and bound to the cranium by the strongest adhesions. The tunica arachnoides is occasionally found thickened and opake, and the pia mater remarkably vascular; the veins are not only turgid, but there often appears, also, high arterial action, evidenced by a bright vermilion tint, scarlet extravasations, the membrane seeming as it were bloodshot. Between the pia mater and arachnoides, there is often observed serous effusion, colourless, turbid, bloody, and even mixed with streaks of coagulable lymph.

The substance of the brain is often unusually firm, and when cut into, the numerous points of blood shew that the divided vessels are enlarged. The ventricles are often enlarged, and contain serous fluid in considerable quantity.

" For the most part, extravasated blood is found within the cranium, sometimes between the membranes, sometimes in the substance of the brain. Sometimes, bursting the walls and floor of the ventricles, we find a great effusion of blood both in the substance of the brain and in the ventricles. Coagulated blood has been found in various parts of the cerebrum and cerebellum of the same subject; and it has been found in the theca vertebrarum. I have never seen extravasation of blood on the surface of the brain, which did not appear to have arisen

from a rupture of its substance; but in a case communicated to me by a friend, the blood seemed to have flowed from a number of the smaller arteries of the pia mater."—" When blood is extravasated into the substance of the brain, it is contained in a cavity, the walls of which are uneven and ragged; and in this irregular cavity, the substance of the brain is generally so mixed with blood, that we cannot wash the blood away without carrying along with it portions of the medullary matter."

He has seen the basilar and internal carotid arteries considerably enlarged. But the extravasation of blood seldom if ever is found to proceed from the rupture of a considerable branch; it has never occurred to his own observation. He denies the general or frequent existence of aneurisms, ossification, or other evident diseased state of the arteries of the brain, as the predisposing cause of these effusions and extravasations of blood in the brain, which almost always arise from the minuter vessels of its substance.

" It is not by the scalpel alone, that the anatomist can demonstrate the state of the vessels which pour the blood into the substance of the brain during apoplexy; but in many cases after he has exposed the seat of the extravasation, he may, by means of a syringe, or by a patient use of a camel's-hair pencil, by washing away the broken portions of the brain, and carefully removing the larger masses of the blood, at last, all along the walls of the irregular cavity, show many vessels not larger than a human hair, ending in small clots of blood; and he will sometimes find the same appearance in various and distinct parts of the same brain. Hence it seems, that the bleeding does not depend on erosion, nor is it owing to aneurism, nor ossification, but to a great and simultaneous action of the smaller arteries of a hemisphere, or of the whole brain; an action which, strong as these arteries are, they in general are unable to bear without a rupture of their coats."

Generally, then, the appearances observed in the brain of apoplectics may be thus summed up:—

" The remains of an excited state of the minute arteries of the brain and its membranes, this probably being the most important, as it is the most unvarying appearance; then the

extravasation of blood, probably the consequence of the excited state of the vessels; the turgescence of the venous system; the enlargement of the ventricles, partial and general; and lastly, the serous effusion which is generally found in various parts of the brain, and which would seem to imply previous absorption of the brain. Again, apoplexy," he observes, " is probably not so sudden a disease as is generally thought; or rather the attack appears connected with changes which have been going on for a considerable time. This opinion is supported by the duration of the disordered functions, which denote the apoplectic diathesis, and by the various appearances which are observed in dissection: the substance of the brain is changed in consistence; the membranes have lost their transparency; and we seldom make a dissection after apoplexy, without detecting preternatural serous effusion."

Such is the condition of the brain in the disease distinctively called *sanguineous* apoplexy. The existence of *serous* apoplexy has been by some entirely denied. It is surely of much rarer occurrence; and the symptoms which have been described by authors as distinguishing it, are not to be trusted to, as they have repeatedly been observed in those cases, which dissection has afterwards proved to have been sanguineous. In one case only of the many which have been attended by Dr. Cheyne, have the appearances of serous apoplexy been exhibited on dissection; there was extensive serous effusion, without extravasation or congestion of blood; the substance of the brain was unusually soft; but even here, the pia mater exhibited signs of inflammation, and the increased number of its minute blood-vessels, bore testimony to the co-existence of increased vascular action.

" When we find, after a fatal case of apoplexy, that we can uncover the brain, without the escape of a single drop of blood; that the brain is soft and colourless; the ventricles enlarged, and perhaps distended with serum; the plexus choroides pale; we see a very different disease from what is described in this volume at such length, and which Galen has emphatically characterized, 'multo nimirum sanguine in principium animantis confertum irruente."

He has been told by a celebrated anatomist,

"That the morbid appearances of the brain, after what was considered serous apoplexy, when instantaneously fatal, as when a man walking quietly along has dropt down dead in the streets, have sometimes barely enabled him to give such an account of the dissection, as might seem to explain the patient's death. And in confirmation of this remark, Mr. Fyffe the anatomist informs me, that in several instances he has scarcely been able to detect any morbid appearances in the heads of persons who had died suddenly, as was supposed, of serous apoplexy. Perhaps a little watery effusion on the surface of the brain, between the convolutions, or in the ventricles, was all he had to note."

The fourth letter of Morgagni, which treats of serous apoplexy, contains fifteen or twenty cases, with dissections; but of these only three appear to belong to this variety. Upon the whole, Dr. Cheyne infers, that serous apoplexy, compared with sanguineous, is a very rare disease. We are still left in the dark during the life of our patients, as there are no symptoms sufficiently characteristic of it, to distinguish it from cases of sanguineous congestion and effusion. The aged, the pale, and leucophlegmatic have had the brain lacerated and injected with blood.

These pathological observations are of the utmost importance. It appears that, in almost every case of apoplexy, there is a high excitement of the vessels of the brain; that the effusion of blood, no less than the serous effusion, proceeds generally from minute vessels, from a number of extreme branches, and not from the rupture of any larger trunk or branch; and consequently that much may be done to obviate the diathesis, and when the attack has taken place, to recover the patient, by preventing or moderating the effusion. The most prompt and effectual means of doing this, would seem to be, to lessen the excitement and turgescence of the vascular system, by unloading the vessels, and lessening the propulsive power of the heart and larger arteries. And this is surely the means which the experience of ages has shown to be the most prompt and effectual. The most obvious means of accomplishing our indication, has indeed been calumniated, and by some physicians neglected. Dr. Cheyne has, therefore, at great length, redargued the sub-

ject, and removed, we think, every objection which can be opposed to blood-letting, which he is convinced is not only the most effectual remedy in apoplexy, but much more effectual than all others in use. Our own experience, which has not been inconsiderable, and we may add, we believe, that of all the physicians of this school, entirely coincides with that of our author. Nothing can be more fallacious, than the celebrated observation of Dr. Fothergill. Of those who are attacked with apoplexy, undoubtedly many die, and others who survive remain paralytic in some of their limbs; because the brain has already suffered irreparable injury from effusion of blood, and laceration of its substance; an effect which blood-letting never can occasion, which it never is known to produce in other diseases, and which never follows epistaxis, or other natural hæmorrhagies, however profuse, but which such evacuations are most likely to obviate and prevent. The only case, perhaps, in which we would hesitate to bleed, is serous apoplexy. To the inference drawn from this case, as an argument against blood-letting as a remedy in apoplexy, Dr. Cheyne replies:

"The physicians who object to blood-letting in apoplexy lest the attack should be of the serous kind, are bound first to teach us how to distinguish this species; and secondly, to prove, that blood-letting is injurious to a patient in serous apoplexy; neither of which they have done.

"Cases are recorded, in which the vessels of the brain have been found gorged, or in which blood has been extravasated, in pale, relaxed, emaciated old men and women, who have led a life of temperance, or even abstemiousness; therefore, the existence of serous apoplexy is not determined by the appearance, the age, sex, nor even by the habits of the patient; yet these are the circumstances upon which the diagnostic hitherto has been rested. Most professional readers know how long and how well Morgagni and Portal have studied pathology; yet we in vain seek in the writings of either for certain means of distinguishing serous from sanguineous apoplexy. For the mere practical physician, Portal indeed has cut the knot, by recommending the same treatment in both varieties."

The case of serous apoplexy is comparatively a rare one, and even in it there are marks of vascular excitement. Dr.

Cheyne tells us that he has attended fifty fatal cases of apoplexy, and of these only one was a case of serous apoplexy.

"Now," he observes, "admitting this proportion, that there are fifty cases of sanguineous apoplexy which require the physician's assistance, for one of serous, (and as some distinguished physicians have altogether denied the existence of serous apoplexy, it is probable that I have not overrated the proportion), we may find an argument in favour of blood-letting, which will not be easily overturned upon the relative frequency of the two species of the disease."

In the treatment of apoplexy, we must recollect the urgency of the case, and the cause which is destroying the brain. Our object is to diminish vascular excitement, and to prevent or moderate a destructive effusion, which would put an end to life; and, as in pneumonia, we must persevere till this object is accomplished, or till there is no longer hope of accomplishing it. A few ounces of blood will not do this in either case. We must bleed largely, and repeat the operation according to the necessity of the case. "It ought to be known," says our author, "that from six to eight pounds of blood have been taken from a person by no means robust, before the disease, which ended favourably, began to yield."

The other remedies which are commonly employed in apoplexy are emetics, purges, and blisters. On each of these Dr. Cheyne has favoured us with excellent remarks. He enters into the argument on the subject of emetics, and discusses it at considerable length. He has shown, we think, in a very satisfactory manner, that where the stomach is evidently overloaded by recent crapulence, this evacuation, excited by the most gentle means, may become necessary and useful; but that generally emetics are improper remedies in apoplexy. Purgatives are, on every principle of reason and experience, to be preferred for relieving the stomach and primæ viæ in this disease. Of blisters, his experience does not speak very highly. They do not seem very applicable, more particularly in the beginning of apoplectic attack; and though he has often had recourse to them, he has never distinctly seen a patient in apoplexy relieved by blistering.

Having concluded his general history of apoplexy, and

thrown much interesting light on the pathology and the therapeutics of this disease, our author proceeds to the relation of his cases and dissections, twenty-three in number, which is followed up by a luminous and instructive commentary. But these observations, however valuable, our limits oblige us to pass over for the present.

We must, however, take some notice of our author's opinions on the subject of lethargy. His observations have led him to question the propriety with which Dr. Cullen has classed such diseases as catalepsis, extasis, carus, cataphora, and lethargus with apoplexy, between which and these diseases there is, he thinks, a distinction which should be restored.

A fair and delicate scrofulous girl, Dr. Cheyne had repeatedly seen affected in the following manner: "Her eyes became fixed, and without expression; her eye-lids dropt, but not so completely as to cover the pupil; her complexion did not change; she was to every appearance insensible, and her breathing was so low that it was not to be heard. Her body lay in the position in which it was when the fit seized her. She had no warning of these attacks, which generally lasted some minutes; her friends knew the fit was over when they heard her sigh. She died before she reached her fifteenth year, of an attack of melæna."

Such cases as this, cases of somnolency, of sudden confusion, and loss of memory, with little or no diminution of the power of sense and motion,—cases in which the patient is torpid, and passes much of his time in sleep, so deep that he cannot easily be roused, and when excited to look up, or answer a question, again relapses into profound sleep,—differ widely from apoplexy in many particulars; and, until the time of Dr. Cullen, were carefully distinguished from it, by the names of lethargus, cataphora, and carus.

There are considerable affinities, however, between all these diseases; and, notwithstanding the very judicious observations now before us, there still appears to be much difficulty attached to this inquiry. It may be easy to decide upon a case of lethargus, or even of cataphora, but between carus and apoplexy the diagnosis is obscure, and from the slightest case of lethargy, to carus, or apoplexy, there seems almost an insensible gradation.

"A sudden and permanent loss of recollection, with torpor and somnolency, has been called a paralytic attack; but when unaccompanied with loss of muscular power, or of sensation, it is with more propriety to be considered as lethargic."

Undoubtedly, in such a case, the distinction is evident. But in the extreme case of lethargy, as in carus, the patient is not to be excited by noise, shaking, or even by pricking or pinching the skin. In carus, any attempt to rouse him is unavailing.

Again, in the beginning of apoplexy, the patient may be excited,—he starts when pricked with the lancet, but as the disease advances towards a fatal termination, he becomes more and more insensible, and no kind of excitation affects him. Indeed, Dr. Cheyne has alleged, that it is by no means uncommon to see carus after apoplexy itself; the patient, after a certain time, becoming pale, with a slow depressed pulse, breathing softly, but totally inexcitable. Thus then, lethargy, cataphora, and apoplexy, and carus, are in regular sequence, and sometimes pass into each other; and if we take serous apoplexy, as another link, we shall find the whole subject still perplexed with infinite difficulty.

Generally, however, it appears, that there is not a necessary or constant connexion between lethargic and apoplectic diseases. A propensity to sleep, and a forgetfulness of recent events, are the leading symptoms of lethargy,—symptoms which are often found to occur without any loss of sensation or of voluntary motion; and persons in hemiplegia are often in possession of every intellectual faculty;—their memory may be unimpaired, and so far from being lethargic, we not unfrequently find them restless, and complaining of want of sleep.

It appears also, that these different states of lethargy and apoplexy are represented by different states of the brain. In lethargic complaints, the diseased appearances are most remarkable on the surface of the brain, which is found covered with serous effusion; and the membranes, particularly the arachnoides, under which much of the watery deposition is confined, are opake. There are marks also of previous excitement; and the substance of the brain, especially the cortical part, is flaccid and moist. In apoplexy and palsy, the diseased appearances are sanguineous and serous effusion into the ventricles and substance of the brain.

Observations on the contracted Intestinum Rectum, and the Mode of Treatment, accompanied with Cases illustrative of the different morbid Appearances attendant on the Complaint. To which are subjoined, two Engravings of the Disease. By W. White, Member of the College of Surgeons, London, and one of the Surgeons to the City Infirmary and Dispensary, Bath. Small 8vo. Bath. 1812. pp. 86. two plates.

[From the London Medical and Physical Journal, for May 1812.]

This little practical work is divided into seven sections. The first section consists of general remarks on the contracted rectum. Dr. Sherwen, Mr. White considers as the first person who wrote a history of the contracted rectum; and his paper on the subject was printed in the second volume of the Memoirs of the London Medical Society. It does not appear, however, that Dr. Sherwen was acquainted with the disease in its early stage, or under the form of simple stricture. But, as it is evident that some of the old practical writers were acquainted with this disease, and that Mr. Pott, in his lecture on diseases of the anus, notices stricture of the rectum, Mr. White can mean only that Dr. Sherwen was the first who wrote expressly on the subject. Since the period of Dr. Sherwen's publication, the disease has been frequently noticed; and the profession is particularly indebted to Mr. Copeland for a more correct knowledge of "stricture of the rectum," as elucidated in his "Observations on the principal diseases of the Rectum and Anus," published in 1810. According to Mr. White, the contracted rectum is a rare disease, for, out of 37,000 patients who came under his immediate notice in the course of twenty-four years, only fourteen cases of this disease occurred, or something more than one in two thousand. But Mr. White does not intend to acknowledge or admit its unfrequency, but rather that it has been misunderstood and overlooked. In this stricture-making age, however, the probability is that stricture of the rectum has often been imagined, and that the ruling passion has often created when it intended to describe a natural fact. Be this as it may, the disease occurs often enough,

and is of sufficient importance to interest the public in all investigations concerning it.

The second section contains "Remarks on the Diagnosis of the Disease." The insidious nature and slow progress, the similarity of symptoms arising from other causes affecting the intestinal canal, render the pathognomonic signs of the disease so precarious and uncertain, that we apprehend Mr. White has much benefited the profession by giving the symptoms as they arise.

Those which more particularly indicate the presence of this disease in its early stage are, says Mr. W.

"Habitual costiveness, occasional uneasiness arising from a sense of fulness in the course of the transverse arch of the colon, but more especially towards the termination of the sigmoid flexure; the patient is also sometimes sensible of the aggravation of this symptom, from the quality or quantity of the food which is taken. There is also an uneasiness in the rectum on going to stool, attended with some difficulty in voiding the fæces. As the disease advances, the alvine excretions becomes gradually more scanty; the fæces are smaller figured than those which are natural, and are often discharged with a squirt. After an evacuation, a sensation commonly continues for some time, as if the whole of the fæces had not been expelled, which by degrees goes off, until the next time of going to stool, when a similar sensation occurs."

To these symptoms we shall add a train of phænomena characteristic of this disease, as given by Dr. Robert White in the fourth volume of the memoirs of the London Medical Society.

"When a person somewhat advanced in life, is troubled with frequent constipation, complains of fulness and weight in the stomach, with repeated inclination to discharge the contents, and uneasy rumbling in the belly and distension in the lower part of it, with a sensation of numbness toward the upper part of the sacrum, extending down the rectum; repeated fruitless efforts being also made to pass a stool, attended with a sense of constriction and tenesmus high up in the rectum, and flatus, which seemed to the patient to occupy the intermediate space, bursts forth; clysters failing as well as medicines, and

the complaint unattended with fever or pain; it will be reasonable to expect some mechanical obstruction in the passage."

Dr. Sherwen, in the publication before cited, considers diarrhœa as one of the earliest symptoms of this disease: this, however, Mr. White controverts, and observes that the disease will be found to be in a very advanced stage, whenever a spontaneous diarrhœa takes place. We would particularly press on the attention of our readers the diagnosis of this disorder, because it frequently exists for a long period before it is detected, and acquires a permanence and stability to resist the curative process, which, perhaps, do not appertain to its early stage. It must be distinguished from the schirro-contracted rectum, from schirrous uterus, from diseased prostate, and from dysentery. Dr. Sherwen and Dr. Robert White, in the Memoirs of the London Medical Society, Siebold Dissertatio de Morbis Intestini Recti, Wrisberg de preternaturali Intestini Recti, &c. Mr. Copeland on the Diseases of the Rectum and Anus, with the work now before us, will afford the means of understanding this disease, as far as printed details go: the book of nature must supply the rest.

The third section treats "*of the Modes of Examination, and the usual Morbid Appearances.*"

The actual state of the intestine is only to be ascertained by the *tactus*. Whenever we have a suspicion of the complaint, a manual investigation must be employed.

"This ought to be performed in the most careful and attentive manner, seeing there is a possibility of mistaking the complaint either for a diseased prostate gland or a schirrous uterus, especially if the hardness and tumefaction is attached to the cervix uteri or back part of the vagina. In prosecuting the examination, the first step to be taken is to introduce the finger (oiled) as high up the rectum as possible, at the same time desiring the patient to bear down, as if going to stool. For, if the examination is first made by introducing a bougie, it may happen, that the instrument is pushed between the folds of the intestine, particularly if there should be particular laxity of its internal membrane; and the practitioner may be led to suppose there is a stricture, when in reality none exists. If, however, on introducing the finger, neither stricture nor

induration can be discovered in the rectum, a small-sized bougie must be introduced, and passed as high as the termination of the colon; because there may be a stricture at that part only, although we commonly meet with one, two or three inches lower. And this I believe will generally be the case, when the superior stricture has been of long standing: analogous to what happens in strictures of the urethra.

"Sometimes it happens that the gut is so much contracted as to render the introduction of the finger impracticable, and the passage will only admit a middle-sized urethra bougie, and sometimes only a small probe."

The diminution of the calibre of the rectum is produced by various alterations in its organization. Sometimes there is only a diminution of the diameter of the canal, perhaps, in its whole length; at others, annular strictures are discovered, or schirrus surrounds and fills up its cavity; and sometimes tubercles obstruct the passage. For further observations, and the morbid appearances detected by dissection, we must refer to the pamphlet.

The fourth section contains the Prognosis. When the stricture is simple, does not extend beyond the sigmoid flexure of the colon, and the patient's general health is unimpaired, the prognosis is favorable. If the disorder is of long standing, the intestine much thickened and indurated, and the general health sensibly declining, with sallow countenance, hectic pulse, &c. the prognosis is unfavorable. Mr. White speaks positively here to an important fact: *whether the disease be simple stricture or confirmed schirrus, it is equally fatal if left to itself.* This fact is confirmed by Mr. Copeland.

Section five inquires into the "Cause of the Disease."

Mr. White asserts, that the complaint is produced "by a morbid action essentially different from that of inflammation. It appears (he adds) highly probable that the glandular structure of the rectum may form the predisposing cause of the disease; and it is also presumable, that the accumulation of hardened fæces, and the pressure occasioned by their passage through the intestine, and the violent straining thereby induced, may prove the general exciting cause."

The method of treatment is explained in the sixth section.

As it evident that a dilatation of the contracted passage is the *sine qua non* of the cure, a strong analogy has led to the mechanical means employed in other strictured parts; but these require to be used with discrimination, caution and considerable reserve. The author examines with freedom the methods employed in this case by others; and the opinions and practice of Wiseman, Pearson, Dessault, Sherwen, Rob. White, Darwin, Charles Bell, and Copeland, pass in review.

The treatment of the disease, as laid down by Mr. White, is divided into two branches, general and medical, and local or chirurgical. The great object in both is to keep down pain and irritation while the distensive means are carrying on, or the remedies employing to remove the disease if it have a specific origin. Bougies, &c. are only admissible when they can be employed without creating great pain and irritation; sometimes the stricture, if not longitudinal, may be divided by the knife; emollient enemata are always useful; and, where anodynes are required, the *extractum Hyosciami* is to be preferred, as it has not the property of occasioning constipation.

The seventh section is particularly valuable by its facts, containing fourteen cases of contracted rectum, treated by the author; to which two plates are annexed, shewing the appearance of the diseased intestine as ascertained by dissection.

Case of Palsy cured by Titillation; with some Observations on the Effects of Titillation on the Nervous System; by JAMES WARDROP.—From the novelty of the remedy, the fortunate result, and the ingenuity of Mr. Wardrop's remarks, we are induced to give our readers the principal part of his detail.

[From the London Medical and Physical Journal for September, 1812.]

" A man twenty-three years of age, in the month of April, 1810, when with the army in Portugal, was seized with a fever, which was at that time epidemic among the troops. In consequence of repeated attacks of the disease, he remained for nearly twelve months in the different military hospitals of Coimbra, Lisbon, &c. Besides what he described as the febrile dis-

ease, he had a severe affection of his head, for which he was bled profusely; and, when he returned to Britain, he had a complete paralytic affection of the left side. By living in the country, he regained a good deal of strength; and he was able to move, though in a very small degree, the paralytic arm and hand. Soon after this, the complaint in his head returned with its former violence, but, by copious and repeated bleedings, the giddiness and stupor, which were the chief symptoms, were removed. The paralytic affection of the left side, during this attack, became more complete; and, although his general health improved, yet, in the beginning of August last, notwithstanding the most rigid antiphlogistic treatment, and a continued use of blisters and electricity to the affected side, the limb continued feeble, and his arm and hand perfectly useless.

" When I saw him at this period, which was the middle of August last, eighteen months from the commencement of his illness, he had a marked halt in the left leg, and the left arm hung motionless, not having the power of even producing any sensible movement with any of his fingers. The arm, too, was considerably wasted; the hand œdematous, with occasional pains about his shoulder; his pulse a little full and frequent; his tongue furred, and a tendency to head-ache. After completely emptying his bowels by aloetic purgatives, and cupping him in the neck, by which treatment all the febrile symptoms, and those of determination of blood to the head, seemed to be removed, I began to try the effect of *titillation*.

" The process which was employed to titillate was extremely simple. A person was directed to draw a feather gently over the surface of the skin on the palm of the hand, until it excited laughter; and I purposed that this should be done three or four times a-day.

" On trial, it was found that the desired effect could readily be produced. At first it was found that a considerable time was necessary to excite laughter; but, the more frequently the application was employed, the more quickly it was accomplished. He found, too, that the laughter was produced much more speedily when he was tickled by another person, than when he used the feather himself; and that it was most easily excited,

by applying the feather to the part of the palm where it is marked by the flexion of the thumb.

" The beneficial effects which the titillation produced on the paralytic arm, exceeded my most sanguine expectations.

" After employing it a few days, he began to feel as if the limb were re-animated, a sensation which lasted a short time after each fit of laughter. This sensation was soon followed by a power to produce a sensible and voluntary motion of the fingers, which daily increased; so that in about a month from the commencement of the use of the titillation, he could grasp his hand with a moderate firmness, and move, in like manner, the elbow and shoulder joints. After this period, the strength of the limb was rapidly regained; and in two months from the commencement of this treatment, I met him in the street carrying a bundle under the affected arm.

" The titillation was also used to the lower extremity of the affected side, the feather being applied to the sole of the foot, where it failed not to produce a speedy and great degree of laughter.

" Besides the titillation, it ought to be mentioned, that I also advised him to rub the limbs with the dry hand, morning and evening; and this was done with a view not only of removing the œdematous swelling, but also of producing an additional excitement on the skin. I have seen the patient four months after the above treatment had been employed, when he remained perfectly well. I am aware that the event of any new mode of treatment, in an individual instance, cannot be sufficient to warrant any general conclusion. The case, however, seems to me to afford an interesting illustration of the influence which the functions of the skin have on the nervous system, and to open a channel for observation and experiment, in various diseases of that system, which may lead to important improvements in their treatment. Under that impression, I have ventured to lay these remarks before the public, in their present form; and with the hopes that, by their early communication, the good effects of the practice may be established by the ample experience of others.

" It may be remarked, before concluding these observations, that a good deal of discrimination must necessarily be employed

in selecting cases where titillation is likely to prove beneficial, and in determining the extent in which it may be prudent to employ such a remedy. As long as the paralytic affection seems to depend on an increased quantity of blood in the encephalon, or to arise from any morbid alteration in the structure of the brain, or its coverings, all attempts by titillation, or other means, to recover the power of the paralytic nerves, must be fruitless, if not highly dangerous. Titillation appears to me to be applicable to those cases only where the primary affection is removed, and where the palsy, which was merely one of its symptoms, alone remains. How far titillation may have an effect in alleviating the excruciating pain of *tic douloureux*, sciatica, and some other affections which seem chiefly confined to nerves, experience can alone determine. Titillation, when employed in the treatment of diseases, must, like other powerful remedies, be at first used with caution; and we are enabled to vary the extent of its effects, from tickling to laughter, or even convulsion.

" Every one knows, from personal experience, the two first degrees of its effects. There are, I believe, many instances of its having produced convulsions, and I have heard of one case where it proved fatal. In all probability, the effects of titillation, as a remedy, should be confined to the excitement of laughter, which in old or obstinate cases may be excited even to an immoderate degree.

" It is, perhaps, difficult to determine how far titillation acts directly on the affected nerves, or indirectly through the medium of the brain; the latter appears to be the most probable opinion; and, if this be found to be the case, it may be equally beneficial to titillate the skin of the sound as of the paralytic limb, thus obtaining a more extensive, and a more efficacious, mode of applying titillation as a remedy. The influence of mental emotion on the nervous system, and on the vital and animal functions, has not yet met with that consideration in the treatment of diseases which it appears to merit. The effects of music in some of these diseases, and the variations of the same disease in individuals, under the influence of different mental emotions, prove this influence, and might lead us to expect that great beneficial advantages might be derived, from acting on the minds of those afflicted with such complaints."

The Morbid Anatomy of the Human Gullet, Stomach, and Intestines. *By* ALEXANDER MONRO, jun. M.D. F.R.S.E. Professor of Medicine, Anatomy, and Surgery, in the University of Edinburgh, &c. &c. &c. Large 8vo. Edinburgh, 1811. pp. 592. xxi plates.

[From the London Medical and Physical Journal, for June 1812.]

IN giving an analytical view of this large volume, the distribution of the subjects adopted by the author will be followed;* and though, perhaps, his arrangement is not the most perspicuous possible, yet, by treading in his footsteps, we shall present our readers with a more satisfactory account of his work, than could arise out of any attempted alteration of his plan, according to any notions of our own.

The Introduction of xxv pages, is occupied with general observations on the advantages arising out of this branch of professional study; and in an elucidation of some particular points of importance. The peculiar advantages of the study of *morbid anatomy* he author enumerates under the five following heads.

"1st. This study, by elucidating the nature and progress of diseases connected with a derangement of organic structure, affords a sure foundation upon which an opinion may be grounded, respecting the nature and progress of such diseases.

"2d. As the study proves that parts similar in their structure are subject to the same organic derangements, it exposes the fallacy of many hypotheses which have been propagated respecting organic diseases.

"3d. If parts of a similar structure be subject to similar organic derangements, *morbid anatomy* may tend to unfold the structure of many of the smaller parts of the human body, which have hitherto escaped observation, as it shows that many of these smaller parts are subject to the same organic derange-

* "The arrangement adopted differs a little from that of preceding authors; for, instead of describing in continuance all the organic derangements of the gullet, of the stomach, or of the intestines, I have classed under one head the same diseases in the different parts of the alimentary canal, following rather the arrangement of diseases than parts." *Introd.* xix.

ments as those organs whose structure is obvious to our unassisted senses.

"4th. *Morbid*, like *comparative anatomy*, assists in distinguishing those organs which are essential to life, from others of less importance, and also shows that every part, even of the same bowel, is not equally sensible or equally necessary to the prolongation of life.

"5th. This study points out the uncertainties and deficiencies of many parts of medical science, and particularly as to the method of cure; and hence may pavé the way to a new and improved method of treatment."

These general observations on the advantages arising from examining and understanding the diseased alteration of structure in various parts of the animal frame, are illustrated by the particular examples of diseased liver, hernia, and stricture of the urethra.

"The pressure of the enlarged liver may interrupt the functions of the stomach; or the same cause, impeding the free circulation of the blood through the different bowels of the belly, as well as through the liver itself, may produce a dropsy of the belly. The enlarged liver is the source of still further mischief; by pressing upon the gall ducts it impedes the free passage of the bile into the intestinal canal; and the bile thus obstructed, passes off by another channel, and is taken up by the lymphatic vessels, occasioning jaundice. Nor is this all the evil to be apprehended from the affection we are considering. By means of inflammation, the enlarged liver may be united with the neighbouring parts; and if, in such a situation, an abscess should take place within, the contents of the abscess might be discharged into the general cavity of the belly, into the sacs of the pleura, or even into the lungs.

"The progress of the disease called rupture, or hernia, affords another and very striking example of the importance of the study of *morbid anatomy;* for even the most accurate knowledge of the anatomy of the body, in its sound state, conveys but an imperfect idea of the state of the displaced parts, or of the state of the canal through which the displacement has been propagated. The displaced portion of the intestine pushes be-

fore it the thin, slippery, and elastic, peritonæum, which lines the belly; and, after such an occurrence, the lining soon loses its natural characters, being converted into a dense, lamellated membrane, which frequently contracts adhesions with the neighbouring parts. The intestines, during the different stages of the disease, are more or less twisted, compressed, and often inflamed. By consequence of the displacement, their coats become thicker, as also the neighbouring cellular substance.

"Of the preceding remarks, the disease called stricture of the urethra affords a very strong illustration. In the earlier stages of the (this) disease it only produces a membranous growth, which, in its future progress, is converted into ligament, and even into cartilage; and the neighbouring parts are frequently reduced to the same morbid state. In addition to the pain and difficulty in discharging the urine, the usual concomitants of stricture, the disease often occasions various constitutional symptoms: among these may be enumerated great nervous irritation, despondency, in some cases bordering upon delirium, and in other cases, symptoms similar to those which accompany an ague."

If the examination of diseased parts, *post mortem*, according to the rules of anatomical science, and with a reference to the symptoms during life, had needed a recommendation to those who feel the value and the advantage of collecting facts, Dr. Monro's Introduction would be particularly valuable; and, should there be any professional persons who still doubt the importance of this species of investigation, we refer them to the arguments here employed.

Before we proceed to a view of the contents of this volume, we shall venture a few cursory observations on the language employed in it, as well as in some other works, of the present period.

Morbid anatomy for the anatomy of diseased parts—Morbid poison for the contagious or infectious material generated in certain diseases—Female complaints for the diseases of the female sex—are expressions of doubtful import at best; and would always mislead without a previous paraphrastic explanation. This affected conversion of words, made, perhaps, with

a view to compression, has done much toward destroying the perspicuity of the language of science. According to accepted usage, they express something very different from what is intended. Thus a female complaint is a disease of the feminine gender—morbid anatomy is the science itself in a state of disease. A thousand analogies will prove this to be the precise sense of the language: a gratuitous admission at first, may, in time and by custom, however, give to this phraseology, perhaps, a legitimate establishment. In the fluctuations of taste, many wilder innovations may arise; or we may return to the old standard.

> "Multa renascentur, quæ jam cecidêre; cadentque,
> Quæ nunc sunt in honore vocabula, si volet usus;
> Quem penès arbitrium est, et jus, et norma loquendi."
>
> "Much phrase that now is dead, shall be reviv'd;
> And much shall die, that now is nobly liv'd,
> If custom please; at whose disposing will
> The pow'r and rule of speaking resteth still."

The practice of others has usually been brought in excuse of employing what is incorrect in itself. Thus far Dr. Monro is supported in the title of his work; but we think it strange that a professor in a liberal and learned science, at one of the first schools in the world for teaching that science, should employ colloquial barbarisms in place of the language which the science itself furnishes, and which all *his* readers would better understand. Why should the disgusting term *gullet* be substituted for the naturalized œsophagus? The doctor apologises for his language on the ground of his being employed on subjects of more importance. Perhaps he is so employed; but he should be aware that neatness, perspicuity, and even elegance, of composition, are compatible with, and essential to, the success of his labours, when he presents those labours to the public. The language of his dissecting room, like his dissecting dress, may be negligent and coarse; but we should be hurt to descry the professor of medicine, anatomy, and surgery, issuing into the streets and public places enveloped in a greasy gown, and an old leathern travelling cap on his head. If the language of this volume is redundant without perspicuity, if familiar without ease, the author has injured medical literature by his example;

"The *better sort* should set before them
A grace, a manner, a decorum;
Something that gives their actions light,
Not only makes them great, but bright."——PRIOR.

Having ventured thus far, *en passant*, on a subject we deem of some importance, especially in this age of invention and coinage, when new terms start up daily, to "fright the language from its propriety," we proceed to the body of the work.

The first chapter consists of "General Observations upon the Structure and Morbid Anatomy of the Alimentary Canal," preliminary to the particular objects of the volume, which are classed under the six following heads.

1. Comprehends an explanation of the morbid effects which have resulted from hurtful substances swallowed by design or accident.

2. Organic affections peculiar to the coats of the alimentary canal.

3. The nature and distressing consequences of the displacement of a part of the alimentary canal.

4. An explanation of the various mal-conformations of the alimentary canal.

5. Description of worms which occasionally infest the alimentary canal.

6. An enumeration of the causes which lead to an enlargement of those neighbouring organs, which, by pressing upon the alimentary canal, prove a mechanical obstruction to the progress of its contents.

Our readers will perceive the rich and interesting harvest which the subject promises, and expectation will not be disappointed.

"The greater part of the diseases of the alimentary canal, included in the above six classes, tend to obstruct the passage of the aliment to a greater or less degree; creating, in some instances, a permanent, in others only a temporary, obstruction.

"The permanent obstruction, by stricture, is of slow growth, but constant in its operation, and becomes greater and greater, either from the gradual increase of the concretion or tumor occasioning it, or from the gradual approximation of the opposite sides of the canal; whereas, the stricture from spasm comes on

suddenly, is generally of short duration, and the spasmodic contraction frequently goes off spontaneously, or after the exhibition of proper remedies.

"But upon some occasions the spasmodic contraction is not removed during life; and upon dissection we find the muscular coat of an unnatural hardness.

"There is in some constitutions a remarkable disposition to diseases which obstruct the *alimentary canal.* I have met with several patients who have been so unfortunate as to labour under two very different causes of obstruction at the same time, or in succession.

"The symptoms, originating from very different causes of obstruction, are in some respects similar; the patient suffers much from a severe tightness, pain, and soreness, in the diseased part.

"The mucous membrane being much irritated, the mucus, which is secreted in an extraordinary quantity, instead of being a mild fluid, becomes thin, of a light green or yellow colour, and acquires an unnatural acrimony, so as sometimes to excoriate the neighbouring parts; and it is discharged in considerable quantity from the mouth, by hawking, or along with the fæces.

"Those mucous glands and their ducts, which are situated above the seat of the obstruction, being much irritated, attain an unnatural size, whilst those under it are not at all affected.

"The above symptoms are occasionally aggravated by catching cold;* and, in these circumstances, and from the irritable nature of the patient's constitution, an unnatural local spasmodic contraction is sometimes excited, which aggravates his sufferings.

* We would be glad to see the popular and vague term, *catching cold,* expelled from works of science. What does it strictly and truly mean? Sometimes it meets us in the form of catarrh, or bronchitis; now it limps in rheumatism, suffocates in asthma, and our Professor employs it to aggravate the symptoms of obstruction in the intestinal canal. In short, it is the scape-goat of all our sins, with respect to the cause of disease. What is the cause of my complaint says the patient? The reply of the nurse, the apothecary, the surgeon, the physician, and the professor, is, you have *caught cold.* Thus have we gone on from age to age, but surely it is now time to question the validity of this catching cold. It may continue to be the shibboleth of old women, but the term should be discarded by men of sense and science.

"The constitution is at first but little affected, but, in consequence of the continuance of the obstructing cause, the digestive powers are much impaired. The patient loaths his food; what is taken becomes acid; he loses his spirits; and is occasionally very costive; but at other times much weakened by a bilious diarrhœa. He becomes weak and emaciated, and has a quick pulse: he suffers after a time very great pain, owing to the distention by air.

"The *air* accumulated in the stomach and intestines, sometimes distends these organs to such an uncommon size, that the convolutions of the intestines may be distinctly felt through the parietes of the abdomen.

"I have seen the stomach and intestines distended to two or three times their natural size, and sometimes even ruptured by unnatural distention.

"It has been matter of dispute among physiologists, from what source the air is derived.

"It is not compatible with the object and limits of this volume, to enter at large into this question, which has afforded matter for so much controversy. I shall therefore only observe, that, as far as I can judge, a part of it is swallowed, a part is formed by secretion, and a part is generated by fermentation.

"As we sometimes meet with cases, where the distention, although so considerable as to occasion great pain and uneasiness, yet has gone off gradually, and without the discharge of air by the mouth or anus, it is probable that part of the accumulated air has been taken up by the absorbent vessels.

"From the abdomen being tense like a drum, where the stomach and intestines are distended by air, the name tympanites has been employed by nosologists, to express this morbid state.

"As the air sometimes escapes from the intestines into the general cavity of the abdomen, in consequence of wounds, ulceration, and other diseases, nosologists also describe the tympanites abdominalis.

"It is of some moment to distinguish the one species of tympany from the other, the former being a much less dangerous affection than the latter.

"The abdominal tympany comes on suddenly, the belly is

uniformly distended in whatever posture the patient is, and also smooth; the patient is not sensible of wind moving within his bowels, nor does he hear the sound of it; and he is not relieved by passing wind, nor is the distention of the belly diminished by a purgative.

"The intestinal tympany, on the other hand, comes on very gradually, the turns of the intestines may be traced by a careful examination; and the sound of the air, passing from the contracted to the dilated position, may be perceived.

"Besides, the patient is sensible of air moving from one place to another; and, whilst the air passes from one turn of the intestine to another, he suffers pain, the intestines being generally spasmodically straightened. The patient also discharges an unusual quantity of air, upwards and downwards, by which he is much relieved.

"It may not be unnecessary to add, that the intestinal tympany has been mistaken for a dropsy of the belly; and, on the other hand, when a small quantity of water is contained within the cavity of the belly, and the intestines have at the same time been filled by air, the distended intestines float upon the water, and render it difficult, unless the patient be examined in different attitudes, to discover the water within the belly.

"An acute inflammation is sometimes the sequel of the immoderate distention of the stomach and intestines; it is rapidly communicated from one part of the abdomen to another, and very frequently proves fatal in a short time, by terminating in gangrene. But, in other cases, in the vicinity of the obstruction, we find the coats of that portion of the alimentary canal, immediately above the distended part, much thickened, in the same manner as the muscular structure of the heart and of the bladder of urine becomes much thickened, where there is an extraordinary impediment to the exit of the blood or urine.

"In such cases, the muscular fibres become thicker, redder, and stronger, to overcome the unnatural resistance.

"In some cases, the part immediately above the obstruction is extended into a pouch. I have seen the gullet, when an extraneous body has been lodged in it for a considerable time, or when it has been obstructed by any other cause, dilated into a large pouch, which still further obstructed the swallowing,

and also the breathing; and, when that pouch is placed near to the termination of the gullet in the stomach, it may press so much upon the heart and lungs as to derange the functions of these organs.

"In a similar manner, the stomach, when the pylorus has been obstructed, sometimes becomes so prodigiously enlarged, as to reach the pelvis; and they are much dilated, from obstruction in any part of the intestinal canal. I have seen the intestinum rectum, in consequence of obstruction, so dilated, as to be capable of containing a child's head.

"The veins, in the vicinity of the obstruction, sometimes become enlarged, or varicose, and add materially to the bulk of the tumour; so that, when the rectum has been extended in consequence of an unnatural obstruction, it proves an impediment to the free discharge of urine, or the contents of the uterus.

"But the part beneath the seat of the obstruction undergoes a very opposite change; it is always somewhat contracted in its diameter.

"The unnatural pouch, created by the obstruction in the alimentary canal, generally adheres to the neighbouring parts. The distention continuing, some part of the pouch becomes thinner than the rest; the absorbent vessels are roused to act more powerfully than usual, by the stimulus of the unnatural distention; the sac becomes thinner and thinner, and at length bursts, or the unnatural sac is destroyed by ulceration; thus a communication is formed between the neighbouring organs, as between the gullet and windpipe, between the stomach and colon, or between the contiguous turns of the intestinal canal. Through this unnatural communication, the contents of one part may pass into another, which proves, on some occasions, the cause of instant death, as when the contents of the gullet escape into the windpipe; but, in other instances, life is thus prolonged, as when the pylorus is obstructed, and when the contents of the stomach pass directly into the colon."

Having made these remarks, which are applicable to the various causes of obstruction in the *alimentary canal*, the author proceeds to describe, in the second chapter, the organic dis-

eases of the *gullet, stomach, and intestines.* This chapter is divided into six sections, the first of which treats "*of Extraneous Bodies lodged within different Parts of the Alimentary Canal, and of their Effects.*"

In this section are collected many extraordinary instances of substances swallowed, with the effects produced, and dissections of patients who died from this cause. The two following show, very forcibly, how far the alimentary canal can accommodate itself to such intrusions.

"A child, two years of age, swallowed a glass ball, three inches in circumference; it passed through the canal in two days, and did not produce a bad symptom. A female child, three years old, swallowed a pair of compasses, two inches and a half long, and which passed through the alimentary canal in three days, without creating, at the time, or afterwards, a bad symptom."

Section 2.—"*Of Obstructions in the Alimentary Canal, occasioned by Alvine Concretions.*"

In this section is given a more extensive history of human alvine concretions, than is elsewhere to be met with; the materials for which have been principally furnished from the extensive collection of these substances made by Professor Monro, senior: four plates are annexed, which considerably elucidate the subject.

The symptoms indicating the existence of these concretions are stated to be irritation, functions of the stomach and intestines impaired and altered, much griping and sometimes acute pain.

"The pain in the bowels, in some cases fixed to one part, is much more severe upon one occasion than another, especially after taking acids, or food of difficult digestion, and is frequently attended by nausea and vomiting.

"Some patients are much constipated for two or three days, and yet have a constant inclination to go to stool. Others have watery stools, and discharge, along with these, a quantity of viscid ropy mucus, or blood; after which they are much relieved.

"Some patients discharge their stools involuntarily.

"Upon relaxing the parietes of the abdomen, a very hard, painful, globular, tumour, may generally be felt, most frequently in the course of the large intestines. It can seldom be made to change its place within the intestine; but often appears to do so in consequence of the change of place of the intestine which contains it. Hence the change appears greatest when the concretion is within the small intestines or arch of the colon, which, from the length of the mesentery or mesocolon, are very moveable.

"The digestive powers being much impaired, the patient becomes very weak and much emaciated; and, from the continuance of the disease, is reduced to a skeleton.

"The pulse, in the earlier stages of the disease, is but little affected.

"Upon the alvine concretion changing its place, and passing down into the sigmoid flexure of the colon, or into the rectum, it creates excruciating torture in the region of the pelvis, and the bowels become much distended, from the passage being suddenly interrupted, and the patient apprehends instant death."

Several cases are subjoined illustrative of the phænomena, management, and termination, of this species of disease of the intestinal canal; and, from his view of the subject, the Professor thinks he has established the following propositions:

"1. That the greater number of *Alvine Concretions* are made up of fibres, which are intimately matted together, and which probably have been attracted by a central nucleus.

"2. That A. C. occasion a derangement of the functions of the alimentary canal, and create griping, obstinate, and long-continued, colicky pains, which are generally limited to that part of the intestinal canal which contains the concretion, and which are occasionally more severe upon the patient's taking acids, or food of difficult digestion.

"3. That A. C. may generally be felt within the intestines, and that when two or more of these are lodged within the intestines, these may be made to strike against each other.

"4. That A. C. frequently change their situation, and pass down into the rectum, which is thereby much extended, and, when so situated, occasion acute pain and sense of weight in the back part of the pelvis, which is attended by a constant de-

sire to go to stool, which the patient cannot gratify; and they may, by a finger, or by an instrument, introduced into the rectum, be felt within it.

"5. That A. C., formed within the human alimentary canal, are, in some cases, discharged by vomiting, or along with the fæces.

"6. That an A. C., after a certain time, cannot be moved from one portion of the alimentary canal to another, owing to its increase in bulk, to the expansion of the coats, in that part which contains the concretion, into a *sac*, and to the unnatural constriction immediately below the seat of the alvine concretion.

"7. That A. C. must prove a mechanical obstruction to the passage of the aliment through the intestines; and, if proper means be not taken to remove the cause of the obstruction, inflammation follows, which proves fatal.

"8. From the chemical analysis of the concretions, it is evident that alvine concretions are of a very peculiar nature, and different from all vegetable and animal productions; and that solvents, sufficiently powerful to act upon those concretions within the intestines, cannot be employed, with impunity, to give the least hope of decomposing those substances within the body, and evacuating them in the ordinary way.

9. "That, in the earlier stages of the disease, while the concretion may be moved from one part of the intestines to another, all that can be done, is to operate on the bowels, partly through the medium of mechanical action, and partly by lubricating the alimentary canal by the exhibition of proper medicines, in order that the concretion may be discharged along with the fæces, or may descend into the rectum, from which it may be artificially extracted.

"10. That after the disease has been of long standing, and when a sac has been formed, which retains the concretion in a certain place, it cannot be removed, should it be lodged within the colon, but by an incision."

Dr. Monro, sen. had suggested that the incision mentioned in proposition 10, might, under certain circumstances, be employed with a fair chance of saving life; and a case is given in this volume (page 58), in which, it is probable, such an operation would have been successful.

Section 3.—"*Of Calculi of the Tonsils.*"

Of this very uncommon disease two cases are inserted. Of the calculi, three in number, in these cases, a description is given by Professor Jameson, and a chemical analysis by Dr. Thomson.

Section 4.—Treats of the "*Effects of Arsenic.*"

In this section are contained several experiments made on animals with arsenic in its metallic state, and with artificial sulphurets of this substance. From these experiments it appeared "that the metallic arsenic might be taken by dogs with impunity, to the extent of several grains." It produced, however, in these cases, some disturbance in the first passages, and its exhibition was followed by a considerable flow of urine.

Section 5.—"*Of the Effects of Opium.*"

It might be expected that the medium through which opium acts on the system would not now be doubtful; the contrary, however, is the fact. The late Dr. Whytt asserted, that this substance affected the system by the medium of the nerves only. That experimental physiologist Fontana, was equally positive that the blood was the medium. "My father's numerous experiments," says our author, "led him to a different conclusion. He supposes, that opium not only affects the nerves, to which it is primarily applied, but is also absorbed; and, being then mixed with the blood, proves fatal, by its sedative powers upon the nerves of the heart and blood-vessels, and the whole nervous system."

When opium has been swallowed in a deleterious quantity, two distinct sets of symptoms arise. These are "vertigo, torpor, slow and full pulse; breathing, at first quick, difficult, and stertorous, and it gradually becomes slower, so that there is a distinct intermission between the inspirations. The pulse gradually becomes more feeble and slower, as the breathing becomes slower. The patient falls into a profound sleep, from which he does not awake, and dies apoplectic, and sometimes is much convulsed. In other cases, vomiting takes place, and the patient becomes afterwards paralytic."

The great object is to remove the poison from the stomach,

and this is to be done by emetics. Of those, Professor Monro recommends, in preference to all others, sulphate of zinc in solution, in doses of ℈j. to ʒss. The greater part of the section is employed in describing an instrument for conveying fluids into the stomach.

Section 6.—"*Of the Effects of concentrated Mineral Acids.*

Of this section, of half a page, it is only necessary to give the title.

Chapter III. Class 2.

"*Of the Organic Diseases of the Coats of the Intestinal Canal.*"

This chapter is of great importance, as it applies to some of the most formidable and fatal diseases incident to the animal frame. In the distribution of the subject of this chapter, Dr. Monro adopts the following arrangement: 1. Organic derangements of the *villous coat* of the pharynx, gullet, stomach, and intestines. 2. Of the *cellular coat.* 3. Of the mucous coat. 4. Of the peritonæal coat.

The organic morbid alterations of the *villous coat* of the pharynx, gullet, stomach, and intestines, are first explained. (The investigation is confined to that part of the alimentary canal which is included between the pharynx and rectum.) These organic derangements are treated of under the heads inflammation, thickening, ulceration and gangrene, ulceration and erosion from dysentery, milt-like tumor, polypi, sarcomata, watery tumors, fungous tumors, hæmorrhoids, varicose tumors, strictures, where the other coats are not affected, projecting ring of Dr. Baillie, aphthæ, small-pox pustule, deposition of cartilage, formation of bone.

The second section of this chapter (the first section contains only a synopsis of its arrangement) treats of inflammation of the villous coat. There are three facts in this section, which, as having come directly under the author's observance, may not be deemed uninteresting to our readers. It has very generally been understood, that redness and turgescence of the neighbouring arteries are essential to inflammation of the intestines. Dr. Monro has seen, however, "the bowels of patients who died with all the symptoms of inflammation, of a sea-green colour; which colour could not be imputed to putrefaction, as the body was (bodies were) examined a short time after

death." In three cases, the author saw the internal membrane of the pharynx thrown into a state of high inflammation, in consequence of the bite of a mad dog. "*The colour of this inflammation was very peculiar; it was not a brilliant red, such as that produced by vermilion, but the purple red, such as carmine or lake gives.*" In the body of a woman who died of diabetes, he "observed an appearance in the intestines similar to that occasioned by inflammation. Upon a more accurate examination, the red colour was found owing to a *red jelly* effused between the peritonæal and muscular coats of the intestines." In this inflammation of the villous coat, the mucus, instead of being a mild, transparent, viscid fluid, is rendered opake, white, sometimes ropy, and even frothy.

Section 3.—"*Thickening of the Villous Coat.*"

The villous coat, from repeated inflammation, remains in a thickened, spongy, and irritable state; and the common occurrence of effusion of coagulable lymph adds very much to this thickening. The general description is illustrated by cases, and the fact rendered more obvious in Plate XIV. In the Museum of Edinburgh there are several specimens of coagulable lymph being thrown into the cavity of the intestine in such quantity as to take the form of the cavity, and, being discharged per anum, were deemed to be worms of a very unusual kind and size.

Section 4.—"*Ulceration and Erosion of the Villous Coat.*"

This state of disease is often the occasion of establishing preternatural communications between neighbouring parts, from the adhesion preceding the ulcerative process. "The symptoms which denote ulceration of the villous coat, are, slight hot and cold fits; hectic flushing of the face; pain in the part affected, becoming less acute, and *detumefaction* of the belly; pulse fuller and slower, often irregular." In the progress of the disease, "the patient becomes very thin, and very weak, and also very costive, hot, thirsty, and feverish, and in this state languishes for a few months; at length he is affected by stupor and delirium, and dies completely exhausted."

Section 5.—"*Gangrene.*"

"Inflammation of the villous coat rarely terminates in gan-

grene." In this section there are many practical remarks, for which we must refer our readers to the work itself. It also contains a detail of the symptoms of hydrophobia, and a minute history of a case; but, why inserted here, we do not fully see; because, in the fauces, larynx, pharynx, œsophagus, stomach, and the whole of the abdominal viscera, there was not the slightest morbid appearance. In a distant part of the volume we find the "symptoms of mortification of the villous coat." P. 151.

Section 6.—"*Of Inflammation, Ulceration, and Erosion, of the Villous Coat from Dysentery.*"

The history and symptoms of dysentery are here detailed from a manuscript of the author's uncle, Dr. Donald Monro. The concluding part of the section we would gladly insert, as containing some general facts respecting the habits of the mucous membrane; but, when we look to the number of pages yet to go over, we are compelled to forbear.

Section 7.—"*Of the Milt-like Tumor of the Mucous Membrane.*"

This tumor, which our author considers as having entirely escaped the attention of pathologists, he describes as having some analogy to the *anomalous tumor* of his grandfather, to the spongoid inflammation of Burns, and the fungous hæmatodes of Hay; and also to that organic disease of the testes, described by Baillie, under the head of pulpy testicle.

"I have called it, says Dr. M——, milt-like tumor, as it resembles in colour and consistence the milt of many fishes; and have added the words mucous membranes, because it grows only from membranes of that description.

"This species of tumor generally attains so considerable bulk, as to fill and even to distend to an unnatural bulk, the bowel within which it is contained, as I have seen in the case of the bladder of urine; but, in other cases, this tumor grows from a part only of the mucous membranes, lining the bowel.

"This milt-like tumor, in many respects, resembles the milt of fishes; it is of a pale red colour, and it is also nearly of the same consistence, but rather softer, and has an irregular surface, and is covered by a thin membrane, upon which there are a number of vessels filled by red blood.

"This species of tumor very readily falls to pieces, and mixes in part with water, forming a turbid mixture; and is somewhat hardened by being put into strong spirits. It adheres but slightly to the organ from which it grows, by a number of small processes, which insinuate themselves into the villous coat, which has attained an unnatural thickness; and, when the tumor has been detached, the villous coat of the diseased bowel assumes somewhat of a honeycomb appearance, and it is besmeared by several drops of blood, which are derived from the vessels which extended to the tumor being torn.

"The bowel from which such a tumor grows externally, betrays marks of inflammation; there is evidently an unnatural determination of blood to the seat of the disease, the blood-vessels upon the peritonæal coat being not only larger, but also more numerous than in the healthy state.

"There is another peculiarity in the disease, viz. a very remarkable offensive fœtor; and the organ, containing such a tumor, is as much discoloured, and emits as fœtid a smell, as the bowel which has been exposed to the air for several days."

An endeavour is made to give the characteristic marks of this tumor which distinguish it from the tumor to which it was compared in the preceding part of the section, accompanied with a coloured plate, (No. V.) drawn from a case here related by Mr. C. Anderson, of Leith.

Section 8.—"*Of Polypi.*"

In this section the varieties of polypus are described, under those of the pharynx, the *gullet*, and the stomach, with an annexed plate, (No. VI.) taken from the disease of a patient, whose case is given.

Section 9.—"*Steatomatous Tumors.*"

The author has not seen an example of this organic disease of the villous coat of the alimentary canal, of course nothing satisfactory is said on it.

Section 10.—"*Fungus of the Villous Coat of the Alimentary Canal, and of Fungous Tumors connected with that Membrane.*"

This disease is not of infrequent occurrence; the tumors

are generally of a small size, of a very soft consistence, bleed when torn, and are mostly composed of several small lobules. The accompanying symptoms depend much on the situation of the disease. When in the œsophagus, difficult deglutition; and, when in the stomach, indigestion, nausea, and rejection of blood by vomiting; when in the intestines, pain, purging, and discharge of blood by the fæces. An etching, (No. VII.) represents a cluster of fungous tumors, which stretched across the colon, and which, by intercepting the passage, had occasioned considerable distention.

Sections 10 and 11, treat very concisely "*of Hæmorrhoids, and Varicose Tumors.*"

From these sections we can only notice that sometimes varicose veins of the stomach burst, and large quantities of blood are lost; and the historical fact of Copernicus having died of an hæmorrhage from an hæmorrhoidal varix.

Section 13.—"*Stricture occasioned by a transverse Fold of the Villous Coat of the Alimentary Canal.*"

In every case of stricture of the œsophagus, stomach, or intestines, which the author has seen, all the coats of the part affected have been constricted, "so that there was an appearance as if a cord had been drawn very tightly around the diseased part of the alimentary canal." Baillie and Home are particularly referred to, but why are not the later and interesting publications of Copeland and White noticed?

Section 14.—"*Of Aphthæ*," and 15, "*Small-pox Pustules within the Alimentary Canal*," afford no remark that will be at all interesting, except that the pustules of small-pox in the intestines is "an extremely rare" occurrence.

Section 16.—"*Deposition of Cartilage and Bone upon the Villous Coat of the Alimentary Canal.*"

The parts of the canal most subjected to this disease, are the œsophagus, colon, and rectum. When the cartilaginous stricture happens to the rectum, the symptoms are described to be,

First, "a slight difficulty in making water; this is followed by nausea, impaired digestion, colicky pains, tenesmus, and

habitual costiveness. The fæces have not their usual size and shape, but resemble small earth-worms, and are expelled only after a considerable effort; and, when the patient is very costive, blood is sometimes discharged with the excrement; and generally a quantity of fœtid mucus, ichorous, or purulent matter. When the disease has been of some duration, solid fæces cannot pass; and the contents of the intestines are discharged in a liquid form, and even these are passed only after a considerable effort."

We now come to the second general division of this chapter, "*Organic Derangements of the Cellular Coat of the Alimentary Canal*," to which, with an unfortunate anomaly as to the construction of the volume, is continued the term section. According to the synoptic view (p. 111) it contains three subdivisions: phlegmon and its consequences; albuminous tumors; and diseases which originate in the other coverings of the intestines. As we have professed to follow our author precisely, we must, with him, call this second general division section 17. This section contains general observations on inflammation of the cellular coat, and its consequences, abscess, &c. Section 18, is on a subject of considerable novelty and interest, "*the Deposition of albuminous Matter in the cellular Coat of the Alimentary Canal.*"

"This organic derangement is of a very peculiar nature, and very generally affects at the same time different bowels of the abdomen and pelvis, and also the lymphatic glands in the vicinity.

"In the earlier part of the disease, we observe small hard tumors, about the size of a pea, in the cellular substance, which grow inwardly, and which push before them the villous coat.

In the more advanced stages, the diseased bowel attains an unnatural hardness and size, and its coats are prodigiously thickened in some places. Tumors of a pyramidal figure grow inwards from the thickened parietes, by which the diameter of the affected part is much lessened. Upon making a section of the coats, we observe the peritonæal coat somewhat harder, whiter, and thicker, than common, but no appearance of the

cellular and muscular coats, for their place is occupied by the albuminous substance."

The liver, testes, uterus, and lymphatic glands, are all subject to this disease, and have their texture and appearance exceedingly changed by it; as it appears in the liver, it is here the subject of a particular description.

The third subdivision has no more than its title.

The third general division, "*Organic derangements of the muscular Coat of the Alimentary Canal*," making the 19th section, contains inflammation and its consequences, spasm, and palsy. The contraction of the middle of the stomach, as stated by Mr. Home, is admitted to be nearly correct; and, in the part appropriated to spasmodic contractions of the intestines, the species or varieties of cholic, in nosologists, are slightly treated of. From this part we extract the following passage, on the possibility of ascertaining the seat of the contraction by the symptoms.

"Pain in the navel has been supposed to be characteristic of contraction in the *jejunum or ilium;* nervous oppression, torpor, inclination to sleep, when the *stomach is empty;* occasional distention of the abdomen, pain in the right side, which is occasionally very great, and which stretches to the back, and occasionally to the top, of the right shoulder, and which changes its place upon the expulsion of air, slight yellowness of the eyes and countenance, with an irregular and soft pulse, that of the *duodenum;* and pain in the right side, stretching toward the region of the liver, that of the *caput cæcum coli.* Acute twisting pain about the navel, which is not increased on pressure, the dragging inwards of the parietes of the abdomen, which, when pressed, feel hard and knotty, tenesmus, and obstinate costiveness, are pathognomonic symptoms of the painter's cholic."

The muscles of the palsied extremities, consequent on this species of cholic, not only lose their natural size, but have their structure converted into a suety substance; as has also happened in rachitis, in scrofula, and in that disease called osteosarcoma.

The 20th section, still subjected to the anomaly before noticed, constitutes the fourth general division of this chapter,

and comprehends the "*Organic Derangements of the Peritoneal Coat.*" The subdivision of this treats of inflammation—Section 21, small tumors growing from the peritoneal coat—Section 22, ossification of the peritonæum—Section 23, hydatids.

The last of these subdivisions, under the denomination of section 23, treats very fully on the obscure form of animal life in the hydatid; and from which the following corollaries are deduced:

"1st. That hydatids are not peculiar to any one part of the human body, and (but) are most commonly connected with the investing membranes of the liver, ovaria, or kidney.

"2d. That there is no resemblance between the hydatids which are peculiar to quadrupeds, and those of the human body, as is obvious, by comparing the preceding description of the hydatid of the human body, with those of the hydatids of quadrupeds, which have been published by Hartmanus, Tyson, Pallas, Schrœder, Fontana, and E. Horne.

"3d. That there is every reason to conclude that hydatids are animals.

"4th. That observation and experiment have not yet determined in what manner hydatids are generated, or deposited, within certain bowels.

"5th. That, as the smaller hydatids adhere to the inner surface of the larger, the larger hydatids may be called pregnant; or that these animals are multiplied, like some vegetables, by the adhesion of the smaller hydatids to the coats of the larger hydatids.

"6th. That the coats of the bowels containing the hydatids are much more frequently destroyed, than when water only has been collected within them; hence the hydatids escape from their original situation, and sometimes find their way by unnatural passages into the intestines, urinary or biliary canals, windpipe, &c.

"7th. That many patients recover upon the discharge of the hydatids.

"8th. That hydatids may, even when adhering to one of the bowels of the abdomen, be removed by incision, provided there exists an adhesion between that viscus and the parietes of the abdomen."

The fifth general division, called section 24, treats of "*Organic Derangements of all the Coats of the Alimentary Canal.*"

Under this division are investigated the various degrees and situations of stricture of the alimentary canal, scrofula of the intestines, coats of the alimentary canal reduced to a pulpy state, induration of the coats of the intestines, dilatation and rupture of a part of the alimentary canal, organic diseases of the mucous glands of the alimentary canal, scirrhus and cancer of the gullet, stomach, pylorus, and intestines, scirrhous rectum, enlargement of the mucous glands of the alimentary canal.

Three plates accompany this part—No. 8, representing a fatal stricture of the œsosphagus; No. 9, a cancer of the œsophagus; and No. 10, stricture occasioned by cancerous tumors at the cardia, and cancer of the stomach.

The third CHAPTER treats of those obstructions which "*originate from a Displacement of a Portion of the Alimentary Canal.*" These causes of obstruction are *Intus-susceptio*, *Procidentia Ani*, and *Hernia*.

Section 1.—"*Intus-susceptio.*"

This derangement occurs much more frequently in infancy than in advanced life. It is of "two very distinct kinds, that which is unattended by inflammation, and that attended by acute inflammation and its consequences. The former, which occurs generally during infancy, in most cases does not merit the name of a disease, as it does not derange the functions of the alimentary canal; whereas the latter, which may be ranked among the diseases of manhood and old age, is one of the most acute and fatal disorders incident to humanity."

The phenomena that distinguish the intus-susceptio, accompanied with acute symptoms, from inflammation of the intestines excited by other causes, are stated to be,

"The sudden appearance of the symptoms after violent straining at stool, the impossibility of throwing up by the anus as much liquid as in a state of health, together with the sudden appearance of a hard tumor on the left side of the abdomen, and which is painful on pressure."

The progress of the disease is well marked in a case from Professor Monro, sen. with a plate, No. 21.

Connected with this derangement of the intestinal canal, is the discharge of a portion of intestine by stool. Alarming as this appears, it is not always dangerous. An instance is here given of fifteen inches of the ilium being brought away by stool, and the patient having good health for many years after.

Section 2, "*Procidentia Ani*" is so connected with section 1, that it is barely mentioned here.

Section 3, "of *Hernia*," is important and extensive, occupying the volume from page 363 to 542; and is illustrated by several plates.

To give such an analysis of this section as would be at all satifactory to our readers, would occupy more space than can be allowed to it; we must therefore refer to the work itself, and only observe, that the author has brought together much curious matter both anatomical and chirurgical, on every species and variety of the disease.

CHAPTER IV. "*Of Mal-formations of the lower Part of the Alimentary Canal*," is confined to a description of mal-conformations of the rectum, under eight varieties.

1st, And most common, is that in which the rectum is covered (when it should terminate in the anus) by the common teguments, or by a membrane of considerable thickness.

2d, When the membrane which obstructs the rectum is internal.

3d, Unnatural contraction of the rectum.

4th, Where the rectum terminates in the bladder of urine, urethra, vagina, or womb.

5th, The rectum terminating in the vagina, through which the fæces are discharged.

6th, Where the rectum is sometimes entirely *awanting* (wanting.)

7th, Where the rectum opens through the os sacrum.

8th, Where the rectum has been continued through the vagina, and has terminated external to the vulva.

CHAPTER V. "*On the Worms which infest the human Alimentary Canal*," gives a short detail of the history and anatomical structure of the *Tenia solium*, *T. dentata*, *T. lata;* the *Ascaris vermicularis*, *A. lumbricoides;* and the *Trichuris hominis*.

Of the last, the *Trichuris hominis*, as being a more rare species than the former, we shall insert the professor's description.

"The body is about an inch long, and it has a filiform tail, about an inch and a half in length.

"Different authors vary in their opinions respecting the anatomy of this worm. According to some, the animal has a proboscis, which it can eject at pleasure; according to Goeze, that is the penis of the animal.

"The stomach and intestines form a long canal, which proceeds from the head to the extremity of the worm, and is largest at the beginning; is much smaller at the tail of the animal.

"The ovarium, which frequently contains ovula, and a limpid fluid, is a convoluted canal, and similar to that of the female ascaris vermicularis."

We had been made to understand, that after chapter five there was to be a sixth, to consist "of an enumeration of the causes which lead to an enlargement of those neighbouring organs, which, by pressing upon the alimentary canal, prove a mechanical obstruction to the progress of its contents." But we have not found it in its promised place.

Upon the importance of the subject of this volume there will be but one opinion—upon the quality of the materials with which it is formed, there will necessarily exist diversity of estimation—upon the style in which it is written, and upon the employment of low and popular terms in the place of those consecrated to science, we have been free in our animadversions. But, if we should be thought to have fallen into any semblance of severity, we deprecate the charge. Our duty to the profession, and to ourselves, required that we should speak out; and this we have done in the spirit of perfect charity with Professor Monro. When we see men in high stations negligent of appearances in their public acts, we feel for the credit of themselves and of the profession to which they belong; and we tremble for the effect of precedent.

ORIGINAL PAPERS.

Remarks upon the Hydrophobia.

In a letter to Dr. David Hosack, of New-York, from BENJAMIN RUSH, M. D. Professor of Medicine in the University of Pennsylvania.

DEAR SIR,

In the fourth volume of my Medical Inquiries and Observations I have endeavoured to prove, that the disease produced by the bite of rabid animals is a state of fever, seated chiefly in the blood-vessels, but extended in a certain degree, in common with several other states of fever, to the nervous and muscular systems. In the course of the last winter I received a pamphlet from Dr. Schoolbred, a British physician in Calcutta, containing an account of four cases of hydrophobia, the histories and cures of which favour the opinion I have published of the nature of that disease, and of the remedies that are calculated to cure it. The first of these cures was performed by Mr. F. Tymon, the second by Dr. Schoolbred, and the other two, by Dr. Alexander Berry.

The symptoms of the disease as described by the two former gentlemen, strongly marked its febrile nature. They were chilliness, a warm skin, a pulse beating from 104 to 110 strokes in a minute, eyes and face suffused with blood, headach, and occasionally, delirium.

The remedies employed by them were,

1. Bleeding to fainting, or until the pulse was scarcely perceptible. The effects of this remedy were, an ability to swallow liquids; bilious discharges from the bowels; a reduction of the frequency of the pulse; a sense of heat, and a desire to be fanned; and lastly sleep, a relief seldom or never experienced in this disease. The blood drawn in the case described by Dr. Schoolbred, was of a scarlet colour, and such as marks the second grade of malignant fevers.

2. Immediately after bleeding, calomel and opium were given in large quantities; the latter in tincture by way of elys-

ter, and by the mouth, in the form of pills. A blister was applied to the forehead in one of the cases with advantage.

The two cases of cures performed by Dr. Berry were of a milder nature. They yielded to purges, injections, mercury, and blisters, without the aid of blood-letting. I know it is common, not to admit any case of disease, from the bite of a rabid animal, to be hydrophobia, unless all the symptoms recorded in books, attend it. We do not reason in this manner in other febrile diseases. We admit of different degrees of violence and danger in the plague, in the bilious fever, and in the small pox. Why should we not admit them in the fever which has received the name of Hydrophobia?

Upon the two cures performed by Mr. Tymon and Dr. Schoolbred, I shall now make a few remarks.

Pathologists have described what they call a soap bubble pulse, that is, a pulse possessing a transient vigor, and such as seldom requires much bleeding to reduce it. There is in like manner a soap bubble state of fever, apparently violent in its first stage, but which soon passes into the prostrate, typhus, or gangrenous states of fever. The bilious fever in the southern states, we are told, frequently exhibits this transition from an active, to a feeble state of the arterial system. This soap bubble form of disease appears likewise in madness when it occurs in hard drinkers. After one or two bleedings, which are called for by the pulse, the system sinks into a prostrate state, and unless it be restored by prompt and liberal doses of opium, volatile alkali, and in some cases by ardent spirits, the disease sometimes terminates in death. Now the hydrophobia appears to be a fever of this kind. No sooner is the soap bubble form of the disease reduced by bleeding, than the system sinks as it does after bleeding for drunken madness, and calls for the immediate exhibition of all the remedies commonly employed in the prostrate, typhus, and gangrenous states of fever. The loss of blood in Dr. Schoolbred's patient was but forty ounces,—in Mr. Tymon's, a less quantity. But there may be cases in which the disease may come on with such symptoms of prostration, as to forbid bleeding altogether, and to require the immediate exhibition of the most cordial, and stimulating remedies. Again, there may be cases, that will yield to deple-

tion by purging and sweating only, and in which bleeding may be hurtful. There may be cases in which bleeding may be forbidden in the beginning, and called for in the close of the disease; and lastly, there may be cases in which the symptoms may require the loss of the same quantity of blood that is required to cure an inflammatory pleurisy or yellow fever. Exactly the same variety in the symptoms, and in the order of their appearance, also in the indications of cure, occur in fevers from nearly all their causes.—But why have I supposed such cases of Hydrophobia to be possible? They are to be met with in books, and it is only where the remedies have been accurately and promptly accommodated to the state of the system in each of them, that a cure either by accident, or design, has ever been performed of that disease.*

In favour of the practice I have recommended for the cure of hydrophobia, let us recollect the different issue of the treatment of the small pox and gout since they have been brought under the dominion of the unity of fever, compared with their issue while they were considered as specific diseases. Ages passed away in fruitless attempts to discover remedies that should destroy by mixture, or expel from the system, the poison of the small pox. It was by boldly renouncing them all, and treating that disease as a state of fever, by bleeding and cool air, or by cordial remedies according to the state of the system, that Dr. Sydenham obtained a partial victory over it. Paroxysms of the gout have in like manner been relieved and cured by rejecting the use of remedies which have been supposed to destroy a specific acrimony, or a specific morbid action, and by exhibiting the common remedies for fever, suited to its different grades and seats. To expect a cure for hydro-

* Mr. Nicholas Nancrede, student of medicine in our university, has lately favoured the author with a sight of a copy of a dissection of a patient that died of hydrophobia in Paris, in whom marks of considerable inflammation were discovered in the upper part of the medulla spinalis. It is possible disease in that part may be the cause of the difficulty in swallowing liquids, and of the spasmodic affection of the larynx. The above dissection suggests the propriety of drawing blood by cups from the upper part of the spine, or of applying stimulating liniments, blisters, or caustics to it, according to the state of the system.

phobia by any other means, is to renounce not only reason, but all experience in medicine.

I shall conclude my letter by mentioning, that several cases have lately occurred under my notice, or within my knowledge, in which there was good reason to believe, from circumstances, that the excision of the part many days, and in one instance six weeks after the wound was inflicted by a rabid animal, prevented the disease. When patients object to this radical preventive, or to the application of a caustic, perhaps moderate depletion by bleeding, purging and sweating medicines, and low diet, would have the same effect in mitigating the disease, or preventing it altogether, that they have in lessening the violence of the small pox, or preventing an attack of the yellow fever. Cases of the success of the first three of those remedies in seven persons bitten by a mad dog, have been transmitted to us from the high and respectable authority of Dr. Hillary, in his Observations upon the Diseases of Barbadoes.*

From, Dear Sir,
yours truly, and very respectfully,
BENJAMIN RUSH.

Philadelphia, March 15, 1813.

Harrisonburg, (Rockingham, Va.) 25th January, 1813.

To the Editors of the Eclectic Repertory.

GENTLEMEN,

I transmit the following case for insertion in the Eclectic Repertory, if it shall be deemed worthy a place in that excellent periodical work.

I am, yours respectfully,
PEACHEY HARRISON.

James Gordon, aged about seventeen years, was taken on Monday the 27th July, 1812, with a difficulty of swallowing. This sensation seemed, at first, to be felt low down the œso-

* Those persons took at the same time musk, cinnabar and camphor, but from their inefficacy in similar cases when used alone, no benefit can be ascribed to them.

phagus, near the stomach, and gradually to arise to the pharynx. On Tuesday the difficulty of deglutition was increased; had pain in his back, some chilliness and some constriction about the breast: began also to feel a degree of immobility of the lower extremities, which were somewhat cold. Thursday, being still worse, his father applied to me, and gave me in substance the history of the case, above stated; and further added, that he was inclined to think, that the symptoms had been brought on by an excessive use of brimstone in curing the itch. Upon inquiry I found, that the sulphur had been used in April, and that the young man had not been well since, without however having any very distinct cause of complaint. Upon the whole, I supposed his symptoms to be nervous, and that no very great importance was to be attached to them. I sent him some antispasmodic and anodyne medicine, his stomach and bowels, as his father informed me, having been well cleansed the day before, with a dose of the May apple root.

On Saturday the first of August, I saw him; and found his deglutition difficult, and his lower extremities very nearly immoveable. I also discovered a paralytic faltering in his tongue. He referred the obstruction in swallowing, to a point a little above the sternum. Upon examining the fauces, but very slight marks of inflammation were discoverable. In fact he had no pain in attempting to swallow; but still he found it very difficult to do it. There was no soreness or swelling any where about his neck or breast. His pulse varied very little from the natural action; had no feverish heat on his skin; but it exhibited a slight degree of lividness. I candidly confess that I was very much at a loss to account for the symptoms, or what course to pursue in the treatment. That he had paralytic symptoms was obvious, but what was the nature of the affection of his throat? Was it the globus hystericus? What had induced the paralysis? It did not seem to proceed from the brain, for the intellectual functions were quite regular, and continued so till the last moment of life, although the paralysis became universal and almost entire. As to the treatment, I detracted twelve ounces of blood from his arm; gave a cathartic; directed a general warm bath, and, after the operation of the purgative, a continuance of the anodyne medicine.

August 4th, again saw him, and found all his symptoms worse; but especially his difficulty of deglutition. He now discharged considerable quantities of phlegm, and his throat was so closed that scarcely any thing could find way to his stomach. Of this fact I convinced myself by causing him repeatedly to attempt to swallow in my presence. In short, he now exhibited the appearance of one badly choaked. So little had he been able to swallow for a few days past, that he complained of great emptiness at stomach, heart burn, and sometimes flatulent distentions. Discovered no aversion from fluids; had no power to move any part of himself, but his head. His case was now extremely urgent, and his danger seemed obviously to arise from the obstruction of the œsophagus and the consequent inanition. I now came to the resolution, and obtained permission, to ascertain by means of the probang, the cause of obstruction in the throat, and this I did the more especially, because it was generally thought at this time by the family and the neighbours, that the choaking was occasioned by a knot of worms, that had made their way up into the throat. Upon the introduction of the instrument, I immediately became satisfied, that the obstruction was not owing to worms; but to an inflammation of a small portion of the œsophagus, which had so diminished its diameter, that it was with difficulty the instrument was made to pass up and down. He felt a smarting sensation, after the instrument was withdrawn, and for a little while could swallow better. A large blister was applied to his neck, and other means attempted, but in vain. He died in a few hours.—I greatly regret that I had not previously been made acquainted with the plan of introducing a flexible tube into the stomach, with a view to convey food and medicine. For as I am persuaded that he died chiefly from inanition, so I believe this plan might have proved the means of saving his life.

Did the paralysis result from the affection of the throat, or was it independent of that affection? Neither my observation nor my reading is sufficiently extensive to authorize me to determine this question, or to say that this is an usual case in the annals of medicine. If similar cases have occurred, and have been recorded, they have not fallen within the range of my research. Is it not a little remarkable that a general paralysis ac-

companied this affection of the throat? Does not this seem to indicate that some peculiar sympathy exists between the œsophagus and the nervous system? The hydrophobia has been demonstrated* by Dr. Physick, if I rightly recollect, to have its seat in the pharynx; and with how tremendous a train of nervous symptoms it is attended, need not be suggested. Can the absence of hydrophobic symptoms in this case, be owing to the lower situation of the affection in the œsophagus? In short, if it had been seated in the pharynx, would it have been hydrophobia, attended with spasm, instead of paralysis? The condition of his stomach was precisely similar to that said to take place in persons labouring under hydrophobia. I make these inquiries, because I do not believe, since I have discovered the real nature of this affection, that it was ever as high up as the pharynx, or as low down as the stomach. It is easy to see that it would be difficult to determine the precise point of the œsophagus occupied by the affection: while that affection was still slight, and after the dysphagia became considerable, he always referred the obstruction to the same place.

Since writing the above I have received information of a gentleman, who, a few days since, died near Charlottesville, in this State, of dysphagia, accompanied with universal paralysis. I believe this to have been a similar case; and it was, I understand, supposed to be owing to exposure, after a free use of the mercurial ointment. He died within four days after the attack of the disorder.

To the Editors of the Eclectic Repertory.

GENTLEMEN,

When I sent you the communication published in the first number of the third volume of the Eclectic Repertory, descriptive of what I supposed a new method of extracting poisons from the stomach, I was influenced by a desire to propose

* We believe Dr. Harrison has misunderstood Dr. P. and therefore we refer the reader to the Medical Repository of New York, Vol. 5th, No. 1st, Article 1st. EDITORS.

to my medical brethren a method of treatment which might preserve the lives of many unhappy persons, who either by design or accident had swallowed large doses of laudanum or other poisonous substances. If in a single instance I had been instrumental in preventing death, I should have considered myself very happy; and to have withheld a communication which might have been attended with such beneficial effects, would have been in every respect unjustifiable.

I have the pleasure of announcing to you and to your readers, that in several instances which have recently occurred in this city, the practice has been completely successful.—In two cases treated by Dr. Dorsey, in which large quantities of laudanum had been taken, there is great reason to believe that no other mode of treatment would have succeeded in preventing the fatal event. Both patients were saved by injecting warm water into their stomachs, and extracting it again, together with the laudanum, by means of a syringe.

I therefore am happy in having called the attention of the profession to a mode of treatment not before used in this country, at least within my knowledge; but I have now an act of justice to perform, in ascribing the merit of the invention to Dr. Alexander Monro, junior, of Edinburgh, who published it in his inaugural thesis, in A. D. 1797. Of this circumstance I was entirely ignorant when I sent you my paper, and probably should still have remained so, had it not been mentioned in his book of Morbid Anatomy, a work which has but very lately come into my hands.

Very respectfully, I am,

gentlemen, yours, &c.

PHILIP SYNG PHYSICK.

Philadelphia, 20th January, 1813.

Brief Remarks on the Doctrine of the Nourishment of the Fœtus by the Liquor Amnii.

BY N. CHAPMAN, M. D.

[The ensuing article contains a very concise examination of the arguments employed by the late Dr. Darwin, in support of the hypothesis, that the fœtus is nourished by the Liquor Amnii. It was written several years ago, and with no view to publication. Now it is committed to the press, with the hope, that it may contribute to check the progress of what I know to be a great, and so far as regards this country, a spreading error in physiology. My criticism is confined to the arguments of Dr Darwin, because, really they constitute the whole defence of the hypothesis. I have, perhaps, pushed my analysis to an unnecessary extent. But as the hypothesis, since its recent revival has gained some adherents, and promises to become still more generally received, I thought it deserving of such an investigation as might once more expose its fallacies.

This little Essay lays claim to no originality. The highest and only merit to which it pretends, is, that of exhibiting a tolerable synopsis of facts and reasonings which are elsewhere to be met with, but in a shape more mixed and diffused.]

Brief Remarks, &c.

EMINENTLY calculated to excite curiosity, we find the nourishment of the fœtus engaging the researches of the earliest cultivators of medicine, and the collateral branches of philosophy. More than one of the writings of Greece which have descended to us, contain very plausible explanations of this obscure and intricate process. Even now, when we boast, not without reason, of the improvements of our physiological knowledge, we retain substantially these primitive conjectures, and have done little else than to correct some of their grosser errors, and to prune away a few of their most obvious absurdities.

Who originally maintained that the Liquor Amnii is the nutriment of the fœtus, it is not easy to determine. The suggestion does not belong to antiquity. It seems to have been promulgated about the period of the revival of letters in Europe.

An hypothesis, in itself so imposing, could not fail to acquire a numerous train of advocates. When subjected however

to a severer investigation, its errors were exposed and it experienced the fate incident to such fallacies. Deserted by its adherents, it sunk into oblivion. But from this state of derelict-tion, it was, after a lapse of half a century, again revived and ushered into notice by the author of Zoonomia. With all his learning and ingenuity, this celebrated writer has brought to its defence not a single new fact, or additional argument. Yet such is the authority of Darwin, that any hypothesis deliberately sanctioned with his approbation, whatever may be its intrinsic defects, can not be passed over with contumely and neglect. What, therefore, he urges in vindication of the position, that the fœtus is nourished by the Liquor of the Amnios I mean critically to examine, and to attain greater perspicuity, I shall apply my strictures to each link of his argumentation, though not exactly in the order which he has adopted.

1. It is contended by him *that the Liquor Amnii is not an excrementitious fluid.* In this opinion I entirely acquiesce. It cannot be the sweat, the urine, the saliva or the mucus from the nose of the fœtus, as has been ridiculously imagined: nor indeed, can it be any other fœtal production, because it exists in a considerable quantity in the early months of gestation, before the fœtus has a single organ completed by which it could be formed, and as it is found in blighted ovula in which there is no fœtus. It seems, most probably, to be an exhalation from the arteries of the amnios. But does not the known disproportion between the quantity of the fluid, and the size of the fœtus very strongly militate against the supposition that it is intended for the purpose of nutrition? We are told by Haller that very little of it remains in many animals at the time of birth, and that in rabbits, especially, there is none. This fact is fully confirmed by the observations of Harvey, De Graaf, and Monro. In the human species it is clearly ascertained, that the relative proportion of the fluid is far greater in the early than in the latter months of pregnancy. We indeed sometimes meet with cases of parturition where it is totally wanting. These, in consequence, are emphatically termed *dry labours.* But there is another circumstance relating to the Liquor Amnii which strengthens the conclusion I am endeavouring to enforce. It is always in

the last stages of gestation more or less vitiated, and sometimes is exceedingly acrid, and even bloody, putrid, and offensively feculent. Were it really alimentary, as is alleged, is it not certain that it would be supplied in a ratio to the increased demands of the fœtus, and continue throughout a pure, unmixed, uncontaminated fluid?

2. It is contended, *that the Liquor Amnii is a nutritive fluid, as it coagulates by heat, by nitrous acid, and by the spirit of wine, like milk, serum of blood, and other fluids which daily experience evinces to be nutritious.* After what I have said, if it be admitted to be true, it is hardly worth the trouble of inquiry whether or not it possesses the properties here ascribed to it. The truth, however, is, that the preceding account of its properties is totally incorrect. By chemical analysis very accurately made, it is proved that no nutritive substance enters into its composition, neither sugar, oil, nor mucilage. It, on the contrary, yields water holding in solution volatile alkali, muriate of soda, phosphoric acid, some earth, and the red calx of iron. Nor does it coagulate as is represented. When heat is applied to it, or it is mixed with spirit of wine or nitrous acid, the only change it undergoes is that of becoming more turbid.

3. It is contended *that the Liquor Amnii has been actually discovered in the stomach, which he conceives is enough, alone to determine indisputably its use in the fœtal economy.* But let us look into the nature of the evidence, and the sources whence it is derived, before we assent to such an inference. By consulting his work, it will be seen that even Darwin's extensive intelligence and various erudition have brought to aid his inference only two cases, and these of a description the most equivocal and unsatisfactory. They, besides, did not occur in the human species. The first of the two cases is recorded by Slade, an old and fabulous writer of slender repute, who relates that he once detected among the fæces of a fœtal calf some hairs of the same colour of those on its skin, and hence surmises that the calf must have licked them off and then swallowed them with the Liquor Amnii. Not to dwell upon what is unworthy of serious scrutiny, I shall merely remark that a circumstance is in the case presumed which can never happen. Though the fœtus when approaching to maturity has an im-

perfect muscular action, it can not make many or extensive motions. Coiled up, so as to adapt itself to the circumscribed capacity of the uterine cavity, it is unable, even if it had muscular power, to change its position so as to perform the actions here ascribed to it. But though hairs were actually found in the excrements of the calf, it would not necessarily follow that they were swallowed, much less that the Liquor Amnii is the food of the fœtus. We know that there is hardly a viscus, or part of the body, which is not subject occasionally to a growth of hairs. They have been described as existing in the urinary bladder, the omentum, the intestines, and even in the heart and arteries.* The second case occurs in Heister's Compendium of Anatomy, and as it is briefly told, I shall quote it in his own words. "I received," says he, "a full formed perfect fœtus of a cow, inclosed in the uterus and membranes, in cold winter weather, where not only the liquor of the amnios which surrounds the fœtus was frozen, but the same liquor was found frozen in the mouth, œsophagus, and stomach, like one continued substance. The column of ice in the œsophagus was about an inch thick. I happened to see the same another winter." The fallacy of the preceding case is too obvious to require a single remark. Congelation can not take place while the animal is alive. The common temperature of the body will not allow of it. It is, therefore, clear that the column of ice was not formed by a stream of fluid in deglutition. If it happened at all, which is very improbable, it must have been owing to the pressure of the waters into the œsophagus by the expansive force which is exerted in the freezing of fluids.

4. It is contended, *that the meconium found in the bowels of new born infants attests that something has been received into the stomach, and what, he triumphantly demands, can it be except the Liquor Amnii?* To this I reply, that whatever may be the materials of the meconium, that it cannot be the recrement of the fœtal aliment entering by the mouth, as animals of different kinds have been born with it, though the communication with the stomach was wanting. Thus Bellinger, in his excellent Treatise on the Nourishment of the Fœtus,

* Monro.

describes a pig with its mouth entirely impervious, yet had its alimentary canal filled with the *usual contents:* and M. Antonie states in the history of the Academy of Sciences for the year 1763, that he once met with a lamb the "stomach and intestines of which were replete with a glary yellow matter like to excrements, which was without head, heart, lungs, pancreas or *liver.*" In the Edinburgh Medical Essays there is also a case related by Mr. Calder, a surgeon, of a child in whom no passage existed between the stomach and intestines, but still the latter contained "a small modicum" of meconium. The preceding facts, which no doubt might be multiplied by more extensive researches, are quite sufficient to overturn the hypothesis which I am combating. With regard to the meconium, it is to me manifestly the product of the alimentary canal.* It has been supposed to result from the bile poured into the intestines. But how can we reconcile such a supposition with the existence of the meconium previously to the liver being in a state to perform its secretory function, and where, indeed, the animal has been *curtailed* of that organ! By many writers it is remarked that the stomach and bowels of the fœtus contain, during every stage of gestation, more or less of mucous matter, differing however from the Liquor Amnii, in its colour, consistence, and other qualities.† It is this mucus, gradually accumulated, and imperfectly digested, which most likely constitutes the meconium. Can we believe that the meconium is the excrementitious part of the Liquor Amnii, when it is recollected, that though the fœtus continues nine months in utero, it rarely, and perhaps never, except when the sphincter ani is relaxed by death, discharges it. Did the fœtus feed upon the fluid, it would regularly void fæces,

* Haller thought it was the remains of the fluid exhaled by the intestines, as, says he, "I have seen a similar substance in other cavities, and in the vaginal coat of the testicle."

† The stomachs of the youngest fœtuses we can dissect are full of a mucous liquor, which remains of nearly the same consistence all the time of gestation, except that it becomes gradually somewhat more viscous as the fœtus increases. "This has obtained in all the different animals, I have had occasion to dissect. The small guts of fœtuses are full of a glary mucilaginous liquor, which becomes thicker and darker coloured as it descends to the great guts, where it is collected under the name of *meconium.*" Monro.

or the accumulation must be immense. We know, however, that the aggregate of the discharges at birth does not exceed, in quantity, the evacuations of a single day after the child is nourished by food taken into the stomach.

5. It is contended, *that the albumen of the egg has been found in the stomach of the chick in ovo, like the Liquor Amnii in that of the fœtus, and hence he infers, that they are nourished in the same way.* Analogies, as has been well observed, are better adapted to the purposes of illustration than of proof. They can indeed be rarely trusted in the discussions of science. To be admitted, at all, they must be close and pertinent. The analogy, in the present instance, is not of this kind. There is no resemblance between the two fluids. The chemical analysis of the Liquor Amnii I have already mentioned. Its properties would seem to denote it to be, though in fact it is not, excrementitious. It is often too, nauseous and feculent. The albumen, on the contrary, yields saccharine matter and gluten, and therefore is nutritive. Nor is it ever vitiated. During incubation it remains sweet.* These distinctions destroy the force of the analogy. Nevertheless, were it indisputably settled, that the chick is nourished in the manner stated, the argument derived from the analogy would certainly be intitled to some weight. But this has not been done. We have not the slightest evidence of the chick feeding upon the albumen. Nothing has been determined upon the point, except that the stomach of the chick throughout incubation contains a glutinous fluid, which, however, we learn with certainty is the product of the alimentary canal and is incident to the fœtal state of all animals.† Of the objections which I have raised to the Liquor Amnii being the

* Monro's experiments are conclusive on this point. They have been repeated by me and with the same result.

† "The mouth, œsophagus, and ingluvies, are always found moist, but never contain any quantity of liquor that can be collected, or will run out in drops. The bulbous glandular point of the œsophagus immediately above the stomach, or what Peyer calls the *infundibulum*, and the stomach, are full of a liquor, in the *youngest* chick we can *dissect*, and continue full the whole time of incubation. This liquor of the stomach is at first thin and more watery; afterwards it becomes curdy, and at last is always in form of a grayish white mucous." Monro.

pabulum of the fœtus, nearly the whole might be urged with equal effect against the nourishment of the chick by the albumen. But waving these altogether, I shall rest the confutation of the latter hypothesis upon different and specific grounds. Not the weakest of this new series of arguments which I am to submit, is deduced from the particular position of the chick in ovo. Without entering into a detailed description, it is enough for my present purpose to mention that the chick lies on its side, with its neck bent forward, and so much so that its beak is placed between its thighs, and that its head is entirely covered with its upper wing. Thus arranged, it so completely occupies the cavity of the shell, that it can hardly alter its posture. With its head fixed under its wing, how can it, I inquire, drink the albumen? But there is a second impediment to its access to the fluid. The albumen, as well as the vitellus, is held in a distinct membranous sac, with which the chick has no communication. This vesicular conformation is a provision of nature to prevent the escape of the fluid. Were it ruptured, the fluid would flow out, and be mixed with the other contents of the egg. But, the advocate of the hypothesis has himself made, very inadvertently, an acknowledgment which is fatal to his conclusion. We are informed by him, and the fact is confirmed by the observations of others, that the albumen of the egg of the hen is entirely exhausted at the expiration of the eighteenth day of incubation. What is the food of the chick during the residue of its fœtal existence? No other source has been assigned. The chick, therefore, according to him, is left for several days destitute of nourishment. The very time that it requires most food it is totally without it. For those who may require other proof, I will add the result of my own experiments. I have ascertained beyond controversy, that the egg, not fecundated, likewise loses its albumen from the fifteenth to the eighteenth day, and hence the corollary is plain, that the fluid is evaporated by the heat of incubation, and is not consumed by the chick.*

* From what I have observed, it seems, that prior to the twelfth day the albumen suffers no diminution either in the *fecund*, or the *infecund* egg. This I believe is owing to its not having attained sufficient tenuity to escape through the pores of the shell. After this period however it rapidly disappears.

6. It is maintained, *that in those cases of monsters which have been born without the natural passage to the stomach, there invariably exists a vicarious opening, and that this uniformity proclaims the solicitude of nature to provide a passage for the transmission of the Liquor Amnii.* There are two cases adduced in defence of this position. The first of a child with an impervious gullet, who had an opening into the trachea and œsophagus; and the other of a lamb without a head, having an aperture in the lower part of the neck. As these are the only cases which the supporters of the hypothesis have as yet collected, it is presumable that they are all which can be produced. The records of medicine and the musea of Europe are full of examples to the contrary.

The two instances, therefore, which are brought forward by Darwin to establish the position of the universality of the vicarious passage, so far from doing it, must be considered as rare and anomalous exceptions to the rule, and when contrasted with the innumerable examples of an opposite kind, can hardly weigh as the dust in the scale of impartial decision.

With this I conclude the inquiry into the defence of the hypothesis before us. No other argument remains to be noticed. It results, I think, pretty distinctly from the preceding exposition that the Liquor Amnii is not the food of the fœtus. Most of the considerations which warrant this deduction have already been detailed. They may perhaps, however, be presented again with advantage in a view more converged. We say then, that the Liquor Amnii does not answer the purpose imputed to it in the fœtal economy.

1. Because the fluid is not nutritious, being at every stage destitute of such properties, and in the latter periods of gestation is always less pure, and often becomes acrid and exceedingly *putrid*, *feculent*, and *bloody*.

2. Because it exists, for the most part, in an inverse ratio to the age of the fœtus, and its demands for nourishment.

3. Because the fœtus has been born in numerous instances without any passage by which the fluid could be introduced into the alimentary canal.

4. Because, prior to the expiration of the third month, the stomach and intestines of the fœtus are in a soft and pulpy

state, totally unfit to receive, or to perform their appropriate action on any alimentary substance.

It would seem, indeed, that during the growth of the fœtus, few, if any, organic functions are performed. While in this state of existence, the operation which goes on almost exclusively, is the evolution and perfection of its different parts. Till this end is accomplished, all the organs, with one or two exceptions, continue quiescent and inactive. The brain is without consciousness, the nerves without sensation, the muscles without voluntary motion, the stomach without digestion, the intestines without peristaltic action, the lymphatics without absorption, the lungs without respiration, the glands without secretion; and in an equally passive condition are the organs of sense. It is the heart and bloodvessels alone which to any extent execute their functions during uterine existence.*

Whatever, therefore, may be the mode by which the fœtus is nourished, certain it is, that its own organic action is little concerned in the process. Of this, we can require no other proof, in addition to what has already been mentioned, than that it continues to grow, though destitute of some of those organs, without which life, after birth, could not be sustained even for a moment. Cases of undoubted authenticity are recorded where fœtuses have attained to the full size, and in other respects were perfect in their conformation, in which the brain, or heart, or lungs, or one or more of the viscera of the abdomen were wanting. The subsistence of the fœtus is obviously altogether parasitical. The food which it derives from its parent, through the mediation of the placenta, comes to it properly assimilated to enter at once into its organization. By the previous elaboration which it undergoes the aliment is almost wholly divested of the grosser and excrementitious matter, and it is on this account, that the excretions of the fœtus are so inconsiderable in quantity.

Philadelphia, Dec. 25, 1812.

* Saumarez's Physiology.

Observations on the Generation and Introduction of CONTAGION *by Ships. In a letter to the* Editors, *from Col.* J. WILLIAMS, *President of the United States Military Philosophical Society*, &c.

WITHOUT engaging in the controversy relative to the importation of the yellow fever, or its generation, in a foul atmosphere, in any country, I may be permitted to state some facts, and to draw such conclusions as these facts will evidently justify, on the peculiar facility of the importation in sea vessels.

A sea vessel must be made *water*-tight, or it could not float on the sea: it must be closed above as well as below, or it would be liable to be filled by the sea washing over it in a storm, and the cargo must be compactly stowed, or it would change its position, by the tossing or rolling of the ship, and endanger its very existence. From these considerations, it is self-evident, that a ship must be *air*-tight as well as *water*-tight, and a portion of that atmosphere, whether pure or foul, in which the cargo was taken in, would be transported with it to the place of its destination, with this single difference, that if pure, it might, by its confinement, become foul, but if noxious, it might, from that cause, become more offensive, but could never become pure.

To illustrate this statement, and to take a more comprehensive view of the subject, let us examine the construction and general situation of a ship and its cargo, during a voyage from any place within the tropical regions.

Of the Construction of a Ship.

The hull (the only part necessary to be examined in this inquiry) is composed of timber and plank. The frame may be called its skeleton, and the plank its external skin. This description applies to its deck, as well as to its bottom and sides, all being water-tight; and, except by the apertures of the hatchway and forecastle, which are both covered with tarpaulins, and battened down when at sea, neither air nor water can enter or escape. Some, indeed, may force its way through the crevices of the bulk head, when the lower deck is not flush fore and

aft, and some gets through the store room, on the occasional opening of the after scuttle, to the great annoyance of the passengers and people in the cabin and steerage. The ribs of this skeleton are of a certain thickness, and are at a certain distance from each other, so that, between the ceiling (that is, an inside lining of plank nailed on these ribs) and the bottom, there are spaces, the cubical contents of which are equal to the square of these spaces multiplied into the thickness of the ribs.

These form receptacles for all the foul air, especially that which is more ponderous than the external air, and, by its coolness, (being below, and consequently of the temperature of the water) even the same kind of air would be more heavy than the circumambient atmosphere, which, in the West-Indies, is generally from eighty-five to ninety-five degrees. But as it is not necessary that the ceiling of a ship should be tight like its bottom, whatever filth there may be in a liquid state, within the ship, must filtrate through the seams of the ceiling, (which are always open) down to the limbers on each side of the keelson. There are in all ships, holes or notches cut in the floor timbers, close to the bottom, in order to permit what water may be in a ship to run into the well-room, where the pumps are placed; but whatever will not filtrate through these passages must remain in the limbers; and, accordingly, whenever a ship is discharged, a plank of the ceiling on each side of the keelson (which is generally left loose for the purpose) is taken up, and the limbers are cleared of all the filth collected during the voyage.

Every man who has been at sea in a tight ship, has been sensible of the stench of the bilge-water, or the accumulated filth in its liquid form, as soon as she quits the port, owing to its agitation by the ship's motion: and every captain of a ship knows, that the colour of the paint in his cabin becomes changed, precisely as the colour of the paint in privies on shore is well known to be changed by foul exhalations. This is so well understood among seamen, that, to designate a tight ship, they always say, "*she does not make water enough to keep herself sweet;*" and, in many instances, they have a plug in the run abaft, and below the water line, purposely to let in sea water to mix with the foul water within, and raise it high enough to be

pumped out. The pumps, it will be remembered, cannot take up all the water, for there must be a hole at the side of each of them to let the water; enter and, the moment the water is below the superior part of the hole, the air will rush in, and the pump will suck; besides, owing to the position of a ship under a press of sail, or by pitching and rolling in a sea, a great part of the water between the ceiling and bottom does not get to the pump.

Of the Cargo of a Ship.

Our inquiry confines us to the produce of the West-Indies, and of other tropical countries, such as sugar, molasses, cotton, coffee, cocoa, and fruits in general. Rum is sometimes imported, but, since the almost universal use of brandy, this now makes an inconsiderable part; what is usually mixed with other parts of a cargo would not correct the effluvia, but only serve to render them somewhat more volatile.

In loading a ship, the heaviest articles are put lowest, for obvious reasons, that relate to the navigation of a ship. The sugar and molasses then, go into the bottom; the coffee and cocoa go into the wings and the interstices among the casks, and the cotton is stowed above all. Here then, it is evident, that the kernels that may drop from the bags, the oozings from rotten fruit, and the filtrations or leaks from the sugar and molasses, will all find their way to the limbers; and there, in the temperature of at least eighty degrees, go into a state of fermentation, or into bodies capable of imbibing it, while the air, generated by that fermentation, will ascend, and be as much as possible imbibed and retained by the cotton. In proportion as air is generated, the whole will become compressed, and give a great velocity to that which might be forced through crevices, &c. Add to this, the issues from the barrels of provisions, from the casks of water, grown foul by long keeping in stagnant air, of waste of every kind made by the sailors between deck, in short, every thing of a filthy kind will find its way to the bottom, and add to the general fermentable mass, always keeping the hold full of compressed foul air. Even the cleanliness of the cabin accumulates the filth below, by washing every thing down into the hold.

This description of a ship and her cargo is purposely made,

with strict attention to the naked facts; and all the effect of negligence, and want of judgment, which many circumstances would justify much reliance upon, in support of the conclusion, are omitted, because the argument is not intended to rest upon accidental or extraneous particulars. The statement evidently shows, that if the germ of the yellow fever be thus *boxed-up* in the West-Indies, it will remain in the best possible hot-bed, till it be brought to sprout in the neighbourhood of our wharves.

Much has been said about the filth of our docks and slips, but these are covered with water, in the motion of rapid tides, at both flood and ebb, and, at the worst, are exposed to the constant action of an active atmosphere; while, on the contrary, a ship has, for several weeks, the noxious vapour carefully kept in vessels, almost hermetically sealed, without the least chance of dissipation. Let a large diving-bell be placed upon the bottom of a very filthy dock, for the whole time that a ship is coming from the West-Indies, and then we might admit of a comparison between the noxious vapour contained under one and within the other. Or to make a more appropriate, and, perhaps, a more accurate comparison, let one of our mud-machines, said to have been so productive of deleterious air at Brooklyn*, be covered with an air-tight deck, and kept four weeks in a temperature never less than eighty degrees; and then, on a sudden, be opened, and the air let loose in a populous neighbourhood, by no means remarkable for its cleanliness, and some affinity between the cases might be claimed; but even then, the quiet residence in a calm dock would bear no proportion to the agitation of a tempestuous sea.

Every man of any chemical knowledge will admit, that air, as well as other fluids, might, in a calm state, exist in strata, according to their density, just as we might make different strata of water, spirits, alcohol, and ether; yet, all being colourless, the bottle would present to the eye a homogenous appearance; so, of air, where the densities differ in the proportion of one to fifteen, perhaps more, without being distinguishable by sight, a perfectly quiescent state would render the comparison much in favour of the mud-machine. Agitation is very favour-

* Brooklyn is on the east river, opposite to New York, where this essay was written.

able to fermentation, and the intermixture of all the air, generated by the fermentation of various substances, is sure to affect the whole mass with the deleterious quality.

But there is another point of comparison which is not inapplicable to this subject: our climate is the most changeable in the whole world; the different seasons show a range of thermometrical graduation of more than one hundred degrees; and even in the same season, often in the same day, the variations are very great, while in any case, either of heat or cold, the same temperature seldom lasts three days. We have frequently seen a fall of snow and summer heat within the space of a few hours.

In the West Indies, on the contrary, there is a perpetual summer heat, scarcely agitated by the gentle sea and land breezes which, like the flux and reflux of the tide, carry off and bring back the noxious particles contained in it alternately. It is only in the hurricane months that nature restores salubrity by violent atmospherical agitation; this is the season of health; and this is precisely the period when, in the course of commercial arrangements, American vessels are not there.

The foregoing remarks apply generally to the construction of a ship, the nature of its cargo, and the occurrences relative to both in the course of a voyage. But it may not be improper to add a few particulars relative to the passengers and people during the same period. We are very sensible on shore of the importance of personal cleanliness; and this is aided by the conveniences of frequent washing, the means for which, even the most indigent have always at command.

At sea, only salt water can be used for this purpose, owing to the limited quantity and important use of fresh water on ship board. It is often prohibited even to wash the hands and face in fresh water; and it is never allowed for washing of cloathing of any kind. Soap is not miscible with salt water, and is of no use whatever. Of late, indeed, it has been discovered that fuller's earth, mixed with salt water, will render it apparently softer, and it is thence imagined that it answers the purpose of soap. This, in some degree, may be true, since the earth may take up a part of the oleaginous substances, with which foul linen, &c. is usually impregnated. But cloathing washed in salt water, will never become perfectly dry in a

moist atmosphere, owing to the salt which is always retained, and which is perpetually imbibing moisture. For these reasons there is but little washing done by the crew, and none by the passengers, whose linen retains all their sea sick filth. Now let us imagine a case to happen in a house at the sea side, in a warm climate. Suppose that there were but two apartments in this house, just as large as the cabin and steerage of a ship; that the bedsteads were fixed to the walls like cabins, and that, like the cobler's stall, they answered the purpose of every usually inhabited apartment. We allow a beach as large as a ship's deck, and the cooking to be done in the open air; but there must be no pump, and only salt water can be used, except for cooking and drinking, and all the fresh water must be kept in casks during the whole time. What would be the situation of such a family at the end of one month? Would there not be reasonable apprehension that a malignant fever would be generated there? And let me ask, if among our most indigent citizens, who inhabit such places as the yellow fever has appeared in, any house has been found in the situation described. Yet any man who has been a few weeks at sea, will recognize the fidelity of the representation, as it relates to a sea vessel; and who has ever boarded a ship at the instant of her arrival, without feeling an impression of disgust? Yet all good captains take care to make the ship herself as clean as possible, just before they expect to arrive. When in port, the passengers land their baggage, and give out their foul cloathing, and the crew also open their chests to have a general washing; and all the confined effluvium, like that of Pandora's box, is immediately scattered in every direction.

A celebrated French writer on the customs, manners, and general habits in Turkey, (Baron de Tott) in speaking of the plague, has this remarkable passage: " La cause qui la propage et le foyer qui la conserve, se trouvent i'une et l'autre, chez les marchands frippiers à Constantinople." So that, according to this author, if the sale of old clothes were prohibited in that city, that terrible scourge might be arrested in its awful and deadly progress. The application of this remark is too evident to need further observation.

Nothing in this description is intended to apply to ships ar-

riving from more northern countries, nor to ships of war. The writer has, in many instances, witnessed the cleanliness of large ships to and from Europe, and along our own coast; and he has, with great satisfaction, observed the efficacious use of wind sails in this and other harbours. But the necessity of these judicious preventives, under such favourable circumstances, serve more strongly to enforce all the foregoing observations, when applied to such small, confined, and generally unhealthy vessels as are usually employed in the West-India trade, and the infectious kind of cargoes imported in them.

Like causes will produce like effects, and hence it may be concluded that (if all circumstances were equal,) the yellow fever may be generated in one place as well as another; and since so much stress has been laid on the occurrences of a voyage, the foregoing statement goes as fully to support the doctrine of generation as it does of importation.—This is granted: it only remains to show the similarity of the causes; but among these, it will be impossible to place those of the voyage: it will likewise be difficult to show a similar state of the atmosphere for any length of time, in our variable climate, and in that of tropical regions. And there will be allowed some difference between the fermentable nature of the productions of the two countries, and the exhalations emitted from them.

But why should we keep up any controversy upon this subject? There cannot be too many nor too strong motives for cleanliness in our populous cities; why then may not the supporters of the doctrine of generation be indulged in all the means recommended for the prevention of disease of any kind, and in turn, why should not they have an equal degree of complaisance for the supporters of the doctrine of importation? It would not be prudent to leave your house open in the night, because you may have a thief within it, nor ought it to be concluded that all thieves are from without: let us therefore be prepared at all points, and support with equal zeal our quarantine laws and salutary regulations, as well to prevent the importation of the disease from abroad, as to guard against its domestic origin.

MEDICAL AND PHILOSOPHICAL INTELLIGENCE.

REPORT OF THE PROGRESS OF CHEMISTRY.

[From the Monthly Magazine, for January 1813]

The interesting experiments of M. Kirchoff, of St. Petersburgh, proving that starch may be converted into sugar by the action of dilute sulphuric acid, have been eagerly repeated by the most distinguished chemical philosophers in Europe. This singular conversion is produced by boiling 100 parts of starch with 400 of water, and from two to eight parts of strong sulphuric acid, in an unglazed earthen vessel for a period of from 24 to 36 hours, constantly stirring the mixture during the first hour, (after which it becomes more fluid) and carefully maintaining the original quantity of water by adding more as it is wasted. Upon growing cold the mixture must be neutralized with chalk, and clarified by charcoal; filtrated through flannel, and evaporated to the consistence of oil. It must then be again cooled, in order to remove its sulphate of lime, and the clear liquor, if further gently evaporated, will yield about 100 parts of gummy syrup of the specific gravity of 1·295, easily susceptible of vinous fermentation, and when separated from the gum, which in general forms no less than a fifth part of it, capable of being crystallized, and applied to all the common purposes of native sugar. With the rationale of this very important transmutation we are not yet acquainted. It is plain, however, that the acid still exists undecomposed, and there is reason to believe that the quantity of water is increased. The probability therefore is that the agency of the acid is exerted in abstracting from the starch a part of its hydrogen and oxygen, in the proportions requisite to form the excess of water, and in thus enabling its remaining principles to be in such a way arranged as to induce the extraordinary change effected.

An easy method of comparing the quantity of light which bodies emit on burning, has been given us by Mr. NICHOLSON. He says, that, when the shadows of the same object, projected on a wall by two lights, are equally dark, the lights themselves are equally intense—that, if not, the darkest shadow will be

projected by the interruption of the brightest of the lights; and that, if this brightest light be then removed farther from the wall till both shadows become equally dark, and the distances of the lights from the wall be in that situation measured, the intensity of each will be in proportion to the square of its distance. For example, if two lights give shadows equally black or dark, when their distances from the wall are respectively five and seven feet, the intensity, or quantity of light emitted from them, will be respectively as 25 (or 5×5) and 49 (or 7×7).

A heavy viscid oil, possessing the flavour of hops united with the odour of nutmegs, and burning with a greenish flame, has been produced by transmitting chlorine gas through oil of turpentine.

From a series of elaborate experiments by M. BERZELIUS, of Stockholm, now in London, we have additional reason to believe that the base of silica is of a metalline nature, the ingenious professor having succeeded in uniting it to iron and other metals, without impairing their metalline properties.

From the same able source, we expect soon to have the pleasure of announcing to the scientific world, the publication of some observations upon Nomenclature, a work which, owing to the rapid strides of modern chemistry, is already very much required.

The proposal of Sir Humphry Davy to introduce into the processes of bleaching calicoes and linens, the use of oxymuriate of magnesia, has experienced but slight attention, on account of the enormous price of this new agent, when compared with that of oxymuriate of lime; a substance which has been successfully employed for many years, and which, moreover, has been proved devoid of those injurious qualities, even when in a concentrated state, from whose supposed existence the proposition of the celebrated Professor originated.

Mr. Sylvester, of Derby, from having ascertained that the amelioration which rum experiences by being kept for some time in its cask, arises from an union of the gallic acid of the wood with the lead, which new rum generally contains, and on which depend its well-known pernicious properties, has been induced to offer this acid as a convenient test for the discovery of lead in cyders, wines, and other liquids, where its presence

is suspected. The same gentleman proposes also to detect arsenic, by the green precipitate occasioned by the addition of an acetate of copper, prepared by decomposing sulphate of copper with acetate of lead. And the presence of corrosive sublimate he recommends to be demonstrated by reducing its mercury to a metallic state upon another metal, (by silvering a golden ring for instance with it) by the agency of galvanism.

A Prussian chemist, by the aid of galvanism, has united the constituent principles of blood into a substance of a reddish colour, which, he asserts, is very similar to the noble vital fluid itself. But that this is utterly impossible, must be the conclusion of any one who weighs the immense difference between vital and chemical action; and we are truly sorry at seeing the ingenuity of so able a gentleman applied in the search of an object so unattainable; for, as Arbuthnot has well observed, "no chemist can make milk or blood of grass."

ROYAL SOCIETY.

Dr. Wollaston, availing himself of the facts reduced to practice by Leslie, in freezing water by means of evaporation in vacuo, described an instrument with which this process might be effected for amusement. Taking a glass tube of any length, such as is used for barometers, making a bulb on each end of it four times its diameter, nearly filling one of these bulbs with water, exhausting the tube of air by boiling the water, and hermetically sealing it like a thermometer, and afterwards plunging the bulb into a mixture of salt and snow, ice would be immediately formed. This might be repeated as often as wished, in a tube three feet long, when the effects of extracting heat from the water would be very distinct to the eye.

Sir Everard Home furnished a short paper, containing some remarks on the solvent glands and gizzards of the different species of cassowary in New South Wales, and the African ostrich. It appears that these organs are always adapted to the native climate and to the quantity of food which the fowl can procure. The length of the intestines, and their magnitude, seem dependent on the like cause. *Tilloch's Philo. Mag.*

On the Composition and use of a new Solution of Ferrum Tartarisatum.—By George Birkbeck, M. D.

In consequence of some experiments on the preparation and properties of ferrum tartarisatum which were mentioned in the tenth number of the London Medical Review, I have been induced frequently to employ it dissolved in water, in various states of disease. Having found that the medicinal efficacy of the iron was equal if not superior to that of any preparation in common use; and likewise that the ferruginous flavour was much less perceivable than in any other compound of an acid and the same quantity of the metal, I have recommended to several practitioners, its employment instead of vinum ferri, mistura ferri composita, or ferrum vitriolatum. From many of these I have received favourable reports of its operation, and I am therefore desirous through this medium still further to extend the knowledge of it.

When water is poured on ferrum tartarisatum prepared according to the imperfect directions contained in the London Pharmacopœia, a portion is dissolved; but as it contains much metallic iron, with very little of the tartrate of iron, a medicine is formed of very trifling power. Even when the ferrum tartarisatum is made according to the judicious directions conveyed in the publication before mentioned, an imperfect, although much more powerful, solution is obtained. This solution is imperfect because liable to decomposition and change, even if kept in close vessels. In order to obviate this imperfection, Mr. Phillips, who has prepared all the solution with which the trials have hitherto been made, has adopted the following process.* Sixty-four parts of cream of tartar are to be mixed with thirty-two of filings of soft iron: to the mass, during the action of the tartar upon the iron, water should be occasionally added; and the digestion is to be continued until it appears, by the test of litmus paper, that the acid is perfectly saturated with iron. To this seven times its weight of water is to be added, which easily dissolves the tartarised iron, when triturated, and the

* Vide an Experimental Examination of the last edition of the Pharmacopœia Londinensis, by Richard Phillips. Page 99.

fluid readily passes through the filtre. For the purpose of saturating any redundant acid, Mr. P. has recently added ammonia, expelling that which may be in excess by subsequent exposure to heat. After standing some time there is a deposit of tartrite of lime from the solution thus prepared, which being removed, it is permanently transparent. The colour of the solution is a deep greenish brown, having, when the acid has been perfectly saturated, very little of the chalybeate taste. It contains about one eighth part of its weight of tartarised iron, or about 3·3 per cent. of peroxide; indeed the quantities have lately been so adjusted by Mr. Phillips, that each fluid ounce of the solution contains exactly sixteen grains of the oxide.

In exhibiting the solutio ferri tartarisati it may be mixed with plain water, or with any of the aromatic waters. It may also be given along with the tonic and bitter vegetable infusions, and with any of the carbonated alkalies, or even pure ammonia, none of these having the power to decompose the tartrite of iron. In all the cases in which iron is indicated this preparation may be employed; and it seems peculiarly well fitted for those disorders in which the nauseous mixture recommended by Dr. Griffiths has been found beneficial. Judging from a few instances in which it has been tried, the combination of this solution with carbonate of ammonia, is likely to afford the greatest advantage to persons labouring under some forms of scrophula, that medicine alone can produce. The dose for children, who take it without difficulty, should vary according to the age from twenty drops to one drachm: adults may take from one drachm to three; or even half an ounce if sufficiently diluted.—*Lond. Med. Rev. No.* 163.

Miscellaneous Communications. By Wm. Simmons, Surgeon.

The first of these gives some account of an anomaly in vaccination. This anomaly is an instance, in Mr. Simmons himself, of receiving the vaccine disease in its regular and efficient form, after having, many years before, passed through the small-pox.

2d. *On the utility of Diuretics in Ulcers situated on the lower Extremities.*—The production of an increased flow of

urine, Mr. Simmons found a powerful auxiliary in the cure of these ulcers, accompanied, generally, with an œdema of the leg.

3d. *On the property of Arsenic in Cancer.*—This is on a subject of such importance, that we shall cite the whole of Mr. Simmons's observations.

"The property of arsenic, taken internally, of allaying the pain of cancer in the ulcerated stage, is already before the public. In the common cancer, and in that variety of cancer to which chimney-sweepers are incident, its effects are nearly similar. In both it improves the quality of the discharge from the ulcer, and alleviates the pain, without inducing stupor or disposition to sleep. Hence it may be considered as an *antalgic* rather than an *anodyne* in such cases.

"In the year 1810 an elderly gentlewoman took the 'liquor arsenicalis' in a case of cancer of the right breast, twice a day for three months. Her sufferings were extreme before she entered on this course; soon, however, the arsenic gave her ease, and, with a few exceptions, kept her easy during the remainder of her life. These exceptions for the most part took place when the tubercles were about to exfoliate; after the exfoliation, the pain again subsided, the hollow granulated; the granulations were florid and healthy-looking, and the secretion from them purulent. By these means, she was kept tolerably easy, compared with what is usually suffered, as she gradually sunk; nor did she experience any of those unpleasant symptoms which are said to arise from the continued exhibition of arsenic.

"As the pain in the common, and in the chimney-sweeper's or soot cancer, is alike alleviated by the internal exhibition of arsenic, may we thence infer the *identity* of the poison in these two diseases? If a different cause can produce the same morbid change of structure in a part, there is no difficulty in conceiving a similarity of function in the subsequent action of the part, however dissimilar the original cause might have been."

4. *On the property of Iron in Cancer.*—Two cases of cancer are related, in which the treatment was conducted according to the suggestions of Mr. Carmichael. In both instances there was a mitigation of the burning sensation spread over the ulcerated

surface; but this was not permanent; and both cases terminated fatally.

5. *Case of Occult Cancer.*—There was nothing unusual in the operation. "On examination of the tumor after extirpation, *it appeared to consist of an homogeneous substance, of a texture so hard as to resound when scraped with the scalpel; but there were no white bands, or intersections of any kind.*"

6. *On the Liquor Ferri Alkalini* (Lond. Pharm. 1809) *in Scrofula.*—In swellings of conglobate glands, and in scrofulous ulceration, Mr. Simmons has exhibited this remedy extensiv ly, and with more benefit than any other preparation of iron or cinchona. He prescribes it in the common dose, largely diluted with water, two or three times a day, according to the sensible effects, increasing or lessening the dose, so as to procure not more than two evacuations from the bowels in twenty-four hours. Besides its operation on the bowels, the kidneys are stimulated to an increased action, and a considerable flow of urine is often the consequence. To these properties of a diuretic, and an aperient, belong those of a tonic, since the appetite and general strength are materially improved by it.

Lond. Med. and Phys. Jour. No. 162.

A new method of curing the Itch has been proposed by Doctor RANQUE, Clinical Professor at the Hotel Dieu at Orleans. It consists in washing the affected parts with a lotion composed of decoctions of opium and staves-acre. This method has the advantage of cleanliness; no unpleasant smell is imparted by the lotion, nor can the least bad accident or inconvenience arise from the use of it, either during the cure or afterwards. This treatment is equally effectual in the most inveterate itch as in the recent disease; in the simple itch, or when it is combined with any other eruptions; in those which are critical, as well as those produced by contagion. Dr. R. has also applied this lotion with great success in herpes and tinea. Four bottles are usually sufficient for a cure in the most inveterate cases.

New London Med. and Phys. Journal, for Sep. 1812.

PHILADELPHIA DISPENSARY.

Managers Elected, January 1813.

William White, President;
Lawrence Seckel,
Robert Blackwell,
Henry Helmuth,
Robert Smith,
Godfrey Haga,
Robert Ralston,
Ebenezer Hazard,
Isaac Snowden,
Joseph Crukshank,
Elliston Perot.
Samuel P. Griffitts, Secretary.

Offieers Elected, January 1813.

Attending Physicians and Surgeons.

Doctor Isaac Cleaver,
Henry Neill,
Samuel Stewart,
George S. Schott,
Joseph G. Shippen,
Joseph Woollens.

Consulting Physicians and Surgeons.

Doctor Benjamin Rush,
Thomas Parke,
Casper Wistar,
Philip S. Physick.

Treasurer.

John Clifford.

Apothecary.

George G. Tresse, at the Dispensary.

HUMANE SOCIETY.

Managers, &c. Elected, March 1813.

Joseph Crukshank, President;
Isaac Snowden, Secretary.

William Leedom,
John Bacon, } Inspectors of the Apparatus.

Samuel P. Griffitts,
Thomas C. James,
Joseph Parrish, } Committee of Correspondence.

Charles Marshall,
Samuel Pancoast, Junr.
Benjamin Thaw,
Charles Penrose,
Matthew L. Bevan.

Joseph P. Hornor, Treasurer.

The Officers of the American Philosophical Society for the present year.

President.

Thomas Jefferson.

Vice Presidents.

Robert Patterson,
Benjamin Smith Barton,
Caspar Wistar.

Secretaries.

Thomas C. James,
Thomas T. Hewson,
Nathaniel Chapman,
Robert M. Patterson.

Counsellors for three years.

William White,
Peter S. Duponçeau,
Jonathan Williams,
Horace Binney.

Counsellors for two years.

Nicholas Collin,
Benjamin Rush,
William Tilghman,
Andrew Ellicot.

Counsellors for one year.

Adam Seybert,
James Gibson,
John H. Brinton,
Edward Pennington.

Curators.

Robert Hare,
Zaccheus Collins,
John R. Smith.

Treasurer and Librarian.

John Vaughan.

FOR THE ECLECTIC REPERTORY.

Statement of Deaths, with the diseases and ages, in the City and Liberties of Philadelphia, from the 1st of January 1812, to the 1st of January 1813.

DISEASES.	Under 1 year	From 1 to 2	From 2 to 5	From 5 to 10	From 10 to 20	From 20 to 30	From 30 to 40	From 40 to 50	From 50 to 60	From 60 to 70	From 70 to 80	From 80 to 90	From 90 to 100	Total
Angina Pectoris	1	0	0	0	0	0	0	0	0	0	0	0	0	1
Asthma	0	1	0	0	1	1	0	1	0	2	0	0	1	7
Abscess	0	0	0	0	1	1	1	0	1	0	0	0	0	4
Aneurism	0	0	0	0	0	0	0	0	1	0	0	0	0	1
Apoplexy	2	0	0	0	0	1	4	6	3	8	7	3	0	34
Atrophy	5	3	2	0	0	0	3	1	1	1	1	0	0	17
Burns	0	0	3	1	0	0	0	0	0	0	0	0	0	4
Cancer	0	0	0	0	0	1	1	1	0	1	0	0	0	4
Casualties	0	0	0	1	0	0	4	1	2	0	1	0	0	9
Catarrh	13	3	0	0	0	0	0	0	0	0	0	1	0	17
Child Bed	0	0	0	0	0	3	0	0	0	0	0	0	0	3
Cholera Morbus	81	63	7	3	0	0	0	1	0	0	2	0	0	157
Cholic	0	0	0	0	0	1	1	1	1	0	1	0	0	5
Consumption of the Lungs	11	3	9	5	22	71	96	52	41	20	8	1	0	339
Convulsions	139	10	9	1	4	5	3	2	2	2	0	0	0	177
Decay	4	5	1	0	2	3	5	8	7	6	7	2	0	50
Diarrhœa	2	3	2	0	1	4	3	2	1	1	1	0	0	20
Dropsy	0	1	0	1	2	3	13	13	9	5	2	0	0	49
of the Breast	0	2	1	1	0	2	2	1	2	3	1	0	0	15
in the Brain	16	13	11	6	2	2	0	0	0	0	0	0	0	50
Drowned	0	0	0	4	3	5	13	3	0	0	0	0	0	28
Dysentery	9	6	1	2	1	2	2	1	0	0	0	0	1	24
Drunkenness	0	0	0	0	0	0	3	1	0	0	0	0	0	4
Diseases in knee joint	0	0	0	0	1	0	0	0	0	0	0	0	0	1
hip joint	0	0	0	0	0	0	1	0	0	0	0	0	0	1
Debility	9	6	2	1	1	1	6	7	5	1	5	1	0	45
Epilepsy	0	0	0	0	0	0	1	2	0	1	0	0	0	4
Erysipelas	2	0	1	0	1	0	0	0	0	0	0	0	0	4
Fracture	0	0	0	0	1	1	0	0	0	0	0	1	0	3
Fever	5	3	3	1	1	3	3	5	3	2	0	0	0	29
Intermittent	0	0	1	0	0	2	0	0	0	1	0	0	0	4
Remittent	0	0	3	0	3	4	5	3	1	1	0	0	0	21
Bilious	1	0	0	0	1	1	1	2	1	1	0	0	0	8
Nervous	0	0	0	0	0	1	1	1	1	0	0	0	0	4
Malignant	0	0	0	1	0	1	1	0	0	0	0	0	0	3
Typhus	1	1	0	1	3	5	13	5	4	3	0	0	0	36
Puerperal	0	0	0	0	0	1	2	1	0	0	0	0	0	4
Hectic	0	0	2	0	1	0	1	0	0	0	0	1	0	5
Scarlet	0	0	0	1	0	0	0	0	0	0	0	0	0	1
Inflammatory	0	0	0	0	2	2	1	0	0	0	0	1	0	6
Mortification and Gangrene	6	0	3	0	1	1	2	1	1	1	1	0	0	17
Gout	0	0	0	0	0	0	0	0	0	1	1	0	0	2
Gravel	0	0	0	0	0	0	0	0	1	0	0	0	0	1
Carried over,	307	123	61	30	55	128	192	122	88	61	38	11	2	1218

DISEASES.	Under 1 year	From 1 to 2	From 2 to 5	From 5 to 10	From 10 to 20	From 20 to 30	From 30 to 40	From 40 to 50	From 50 to 60	From 60 to 70	From 70 to 80	From 80 to 90	From 90 to 100	Total
Brought forward,	307	123	61	30	55	128	192	122	88	61	38	11	2	1218
Hooping Cough	8	8	6	2	0	0	0	0	0	0	0	0	0	24
Hives	11	4	4	1	0	0	0	0	0	0	0	0	0	20
Hernia	0	0	0	0	0	0	0	0	1	0	0	0	0	1
Hæmorrhage	0	0	1	1	1	0	0	0	0	0	0	0	0	3
Hydrophobia	0	0	0	0	1	0	0	0	0	0	0	0	0	1
Inflammation of the Brain	10	1	0	0	0	0	0	0	0	0	0	0	0	11
of the Lungs	1	1	1	0	0	0	1	0	0	0	0	0	0	4
of the Stomach	18	5	1	3	2	0	4	1	1	0	1	0	0	36
of the Bowels	11	2	1	5	1	2	1	1	2	1	0	0	0	27
of the Liver	0	0	0	1	2	5	0	2	4	0	0	0	0	14
Insanity	0	0	0	0	0	1	10	14	0	4	1	0	0	30
Jaundice	0	0	0	1	0	0	0	1	0	1	0	0	0	3
Measles	6	8	3	3	0	0	0	0	0	0	0	0	0	20
Old Age	0	0	0	0	0	0	0	0	0	1	17	21	7	46
Pleurisy	7	3	3	1	4	8	12	10	9	9	2	1	1	70
Palsy	0	0	0	0	0	1	3	1	2	4	11	1	0	23
Rheumatism	0	0	0	0	1	0	1	2	1	1	1	0	0	7
Scrofula	0	1	1	3	0	0	0	0	1	0	0	0	0	6
Sore Throat	9	1	3	1	0	0	1	0	0	0	0	0	0	15
Still Born	142	0	0	0	0	0	0	0	0	0	0	0	0	142
Suicide	0	0	0	0	0	1	1	1	0	0	0	0	0	3
Sudden	1	0	0	1	1	5	12	8	3	0	1	0	0	32
Syphilis	0	0	0	0	0	4	1	0	0	0	0	0	0	5
Strangled	0	0	0	0	0	1	0	0	0	0	0	0	0	1
Teething	5	5	1	0	0	0	0	0	0	0	0	0	0	11
Ulcers	0	0	0	0	1	0	1	0	1	0	0	0	0	3
Wounds	0	0	0	0	0	1	1	1	0	0	0	0	0	3
Worms	0	3	6	0	1	0	0	0	0	0	0	0	0	10
Unknown	1	0	0	0	0	2	4	3	1	0	0	0	0	11
Total,	537	165	92	53	70	159	245	167	114	82	72	34	10	1800

NOTE. Of the above there were 505 males of twenty years and upwards, 419 under twenty years: of females 417, of twenty years and upwards, 381 under twenty years; and 78 children, principally under one year, whose sex is unknown.

It is farther to be noted that no returns have been received at this office since the 11th of July last, from the public burial Ground, at which time the City Commissioners took it into possession. However, it appears from the publication of deaths, by the Clerks and Sextons of the United Episcopal Churches of Christ, St. Peter and St. James, that there were interred since that period in the Public Burial Ground 359, which added to the above number will give a grand Total of 2159.

Deaths in each month of the above period.

	Adults.	Children.
January	114	89
February	98	59
March	87	63
April	128	77
May	85	59
June	76	69
July	68	92
August	60	155
September	59	108
October	51	42
November	51	44
December	36	28
Total	915	885

By order of the Board of Health,

JOHN ALLISON, Clerk.

Health Office, February 18th, 1813.